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SUMMARY

of No. 50 (Vol. XVII, 1)
(284 p., 16 pl., 3 fig.)

I. *Main Articles* (12)

A. Pogo *(Cambridge, Mass.)*. — Calendars on coffin lids from Asyut (6 pl., 4 tables). 6
B. Datta *(Calcutta)*. — On the relation of Mahāvīra to Śrīdhara 25
A. Birkenmayer *(Krakau)*. — Zur Lebensgeschichte und wissenschaftlichen Tätigkeit von Giovanni Fontana (1395 ?-1455 ?) 34
P. Schub *(New York City)*. — A mathematical text by Mordecai Comtino. 54
J. Ferguson *(Glasgow)*. — Notes on the work of Polydore Vergil « De Inventoribus Rerum », edited by E. H. Alexander (1 portr.). 71
K. Maynard *(Cambridge, Mass.)*. — Science in early English literature (1550 to 1650) 94
C. D. Hellman *(New York City)*. — John Bird (1709-1776), mathematical instrument-maker in the Strand (2 portr., 3 fig.) . . 127
G. Sarton *(Cambridge, Mass.)*. — Discovery of conical refraction by W. R. Hamilton and H. Lloyd (1833) (with 9 p. facsimile no. XIV; portrait of H. Lloyd) 154
J. Pelseneer *(Brussels)*. — Gilbert, Bacon, Galilée, Kepler, Harvey et Descartes : leurs relations 171

G. SARTON *(Cambridge, Mass.)*. — Section of the History of Science of the Carnegie Institution 209
J. MILLÀS VALLICROSA *(Madrid)*. — La introducción del cuadrante con cursor en Europa (6 pl.) 218
J. PELSENEER *(Brussels)*. — A propos de notations mathématiques. 259

II. *Notes and Correspondence* (8)

I. *Queries*. — Query No. 18. ANTONIO BENIVIENI (E. R. LONG). — Query No. 19. A. DAVID's letters to F. DE ZACH, P. F. TRIESNECKER and to H. C. SCHUMACHER; ZACH's Ms. (O. SEYDL) . 260
II. *Miscellanea*. — Ghubār numerals (M. C. Welborn). — Critical edition of the collected papers of A. VAN LEEUWENHOEK. — Prix EUGENIO RIGNANO 260
III. *Personalia*. — GOICHI SAWATA. TOYOTA TAKANUKI (Y. MIKAMI) HEINRICH WIELEITNER (G. S.) 264

III. *Reviews* (8)

Asia W. KAMMERER, A. — Pétra et la Nabatène. 2 vol. Paris, GEUTHNER, 1929 (G. SARTON) . . 265
China. DE SAUSSURE, L. — Les origines de l'astronomie chinoise. Paris, MAISONNEUVE, 1930 (A. POGO) 267
Islam. ASÍN PALACIOS, M. — El Islam cristianizado. Madrid, Edit. Plutarco, 1931 (G. SARTON). 271
Philosophy of Science. NORTHROP, F. S. C. — Science and first principles. New York, MACMILLAN, 1931 (H. T. DAVIS) 273
Biology. NEEDHAM, J. — The sceptical biologist. New York, NORTON & Co., 1930 (A. MEYER) 277
Art. FARMER, H. G. — The organ of the ancients. London, REEVES, 1931 (G. SARTON) . . . 278
Medicine. WARTHIN, A. S. — The Physician and the Dance of Death. New York, P. HOEBER, (C. A. KOFOID). 1931. 282
Janus, 1846-48; 1851-52. Neue Ausg. 5 vol., Leipzig, LORENTZ, 1931 (G. SARTON) . . 283

IV. *List of Plates* (15)

1. Coffin of HOUNNOU. 7
2. Coffin lid of HOUNNOU. Right half (head panel).

3. *Id.* Left half (foot panel).
4. Coffin lid of TEFABI. Right half (head panel).
5. *Id.* Left half (foot panel).
6. *Id.* Transversal picture.
7. POLYDORE VERGIL (1470?-1555?). 73
8. JOHN BIRD (1709-1776). 129
9. GEORGE GRAHAM (1673-1751). . . . . .
10. Bust of HUMPHREY LLOYD (1800-1881). 163
11. Quadrant with cursor (from el Libro del cuadrante). . . 219
12. *Id.* (Ms. Brit. Mus., XIth century).
13. *Id.* (Ms. Vatic. Reg., XIth century).
14. Quadrant of AZARQUIEL.
15. Quadrant with the lines of projection (AZARQUIEL).
16. Quadrant with the hour lines (AZARQUIEL).

Calendars on coffin lids from Asyut
(Second half of the third millennium)

ABSTRACT

The coffins of MSAHITI, MAIT, KHITI, pseudo-NAKHITI, HOUNNOU and TEFABI, discovered in Asyut between 1893 and 1903, belong to the IXth or Xth dynasty. The lids of these coffins are decorated with diagonal calendarial tables.

The tables of MSAHITI and TEFABI belong to the 36-column type, the tables of KHITI and HOUNNOU to the 24-column type. Theoretical considerations leading to the constructions of such calendars are outlined. The illusiveness of dating these tables by the rubricated titles of the columns preceding and following the intersemestrial transversal picture is pointed out.

The connection between the upper or sunset row of the diagonal calendars and the decanologues of the southern panels of the ceilings of the XVIIIth and XIXth dynasties is established. The division of the ceilings into two panels separated by a longitudinal inscription, the presence of the Ursa Major group in the center of the northern panel, and of the Orion-Sirius group in the center of the southern panel, result from the layout of diagonal calendars of the Asyut type. The meta-Sothic lists of the ceilings correspond to the astronomical divinities of the 36th column heading the diagonals of the terminal triangle of a complete calendar.

The SENMUT-Ramesseum decanologues are related to the 36-column type of calendars; the SETI-tradition points to a special 24-column type of calendar not found on the available Asyut coffin lids.

1. *The coffins from Asyut.*

The coffin of MSAHITI—the first of the group of six coffins of the Middle Kingdom dealt with in this paper—was found in Asyut, in 1893. A paper by G. DARESSY (1), published in 1900, was the first attempt to interpret the astronomical decoration on the inner surface of the lid. A full description of the MSAHITI

(1) G. DARESSY. Une ancienne liste des décans égyptiens. *Annales du service des antiquités de l'Égypte*, 1, 79-90, 1900.

PLATE A

Coffin of HOUNNOU. The front edge of the lid is at the top on Plates B and C.

(*Mémoires de l'Institut français d'archéologie orientale du Caire*, vol. 24, pl. XXVIII, 1911. From a photostat reproduction.)

PLATE B

Coffin lid of HOUNNOU. Right half (head panel).

(Courtesy of the Musée du Louvre.)

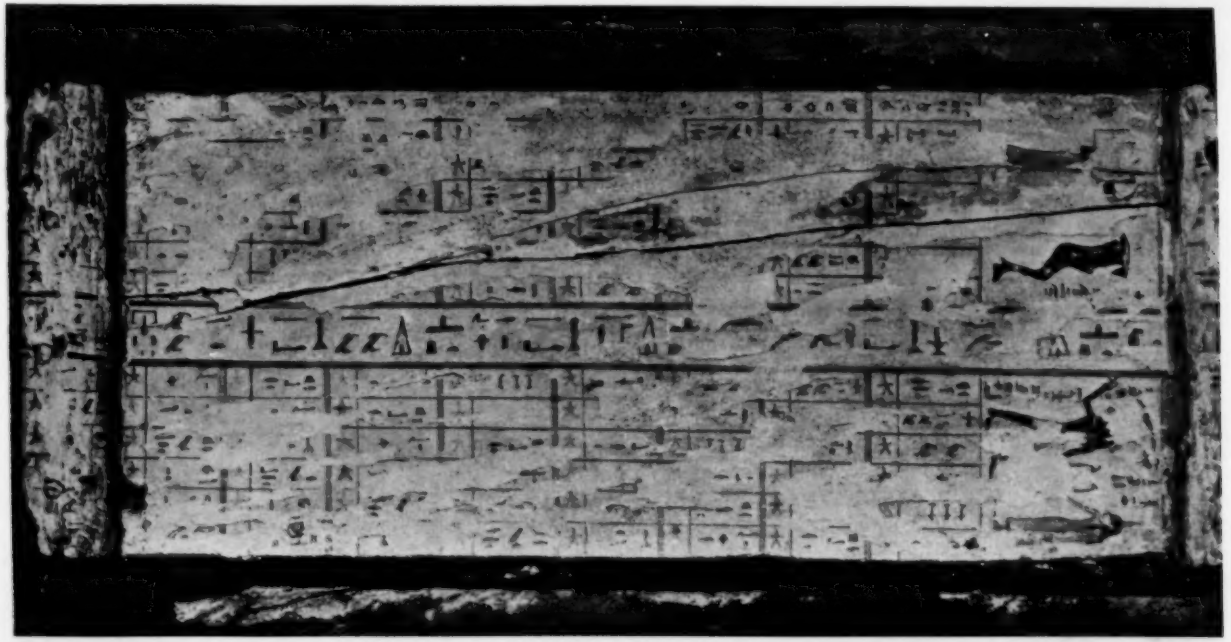


PLATE C

Coffin lid of HOUNNOU. Left half (foot panel).

(Courtesy of the Musée du Louvre.)

PLATE D

Coffin lid of TEFABI. Right half (head panel).

(Courtesy of the Cairo Museum.)

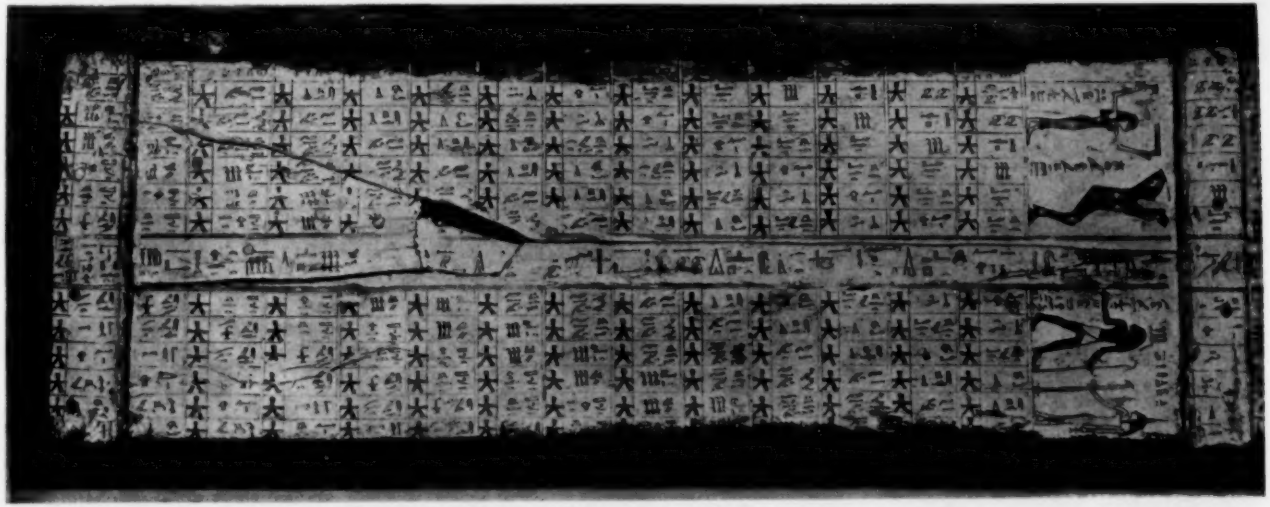
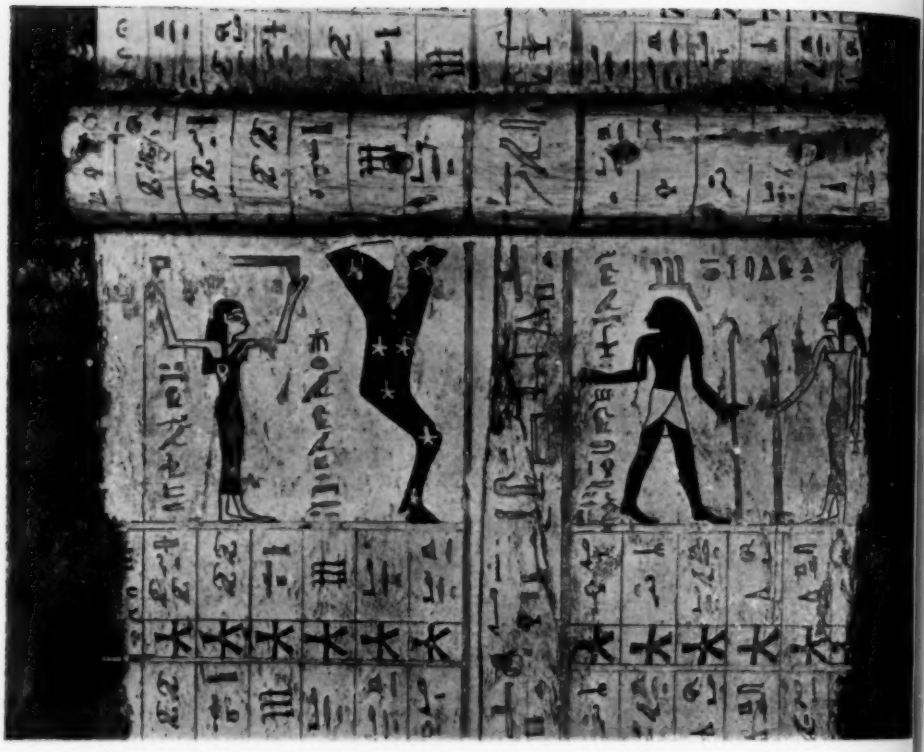


PLATE E

Coffin lid of TEFABI. Left half (foot panel).

(Courtesy of the Cairo Museum,)



Isis, XVII, pl. 6. A. POGO,

PLATE F

Coffin lid of TEFABI. Transversal picture.

(Courtesy of the Cairo Museum.)

coffin (Cairo, No. 28118) was published, in 1906, by P. LACAU (2) who refrained from astronomical comments on the table decorating the inner surface of the lid.

The excavations made at Asyut by CH. PALANQUE, under the direction of E. CHASSINAT, led, before 1903, to the discovery of the coffins of MAIT, KHITI, pseudo-NAKHITI, HOUNNOU, and TEFABI. No astronomical comment on the tables decorating the inner surface of the lids of these coffins was made by E. CHASSINAT, the editor of the memoir (3) published in 1911. K. SETHE repeatedly refers to the Asyut coffins in his papers (4) presented to the Goettingen Society in January 1920.

MSAHITI, MAIT, KHITI, pseudo-NAKHITI, HOUNNOU, and TEFABI lived during the Heracleopolitan period (IXth and Xth dynasties). The six lids belong to their inner coffins; there were no astronomical inscriptions on the outer coffins. The pseudo-NAKHITI inscription is in such a poor state of preservation that E. CHASSINAT contented himself with the remark (5) that it is similar to the MAIT, KHITI, and HOUNNOU (6) inscriptions; it apparently belongs to the 24-column type discussed below.

In 1922 fragments of the coffins of another Asyutian, ḤENY, of about the same period (XIth dynasty?) were discovered; they bear astronomical inscriptions of an entirely different kind; see my paper in *Isis*, XVIII, 1932.

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(2) P. LACAU. Sarcophages antérieurs au nouvel empire, tome 2, 101-128, 1906. (*Catalogue général des antiquités égyptiennes du Musée du Caire*, vol. 33.)

(3) E. CHASSINAT et CH. PALANQUE. Une campagne de fouilles dans la nécropole d'Assiout. *Mémoires de l'Institut français d'archéologie orientale du Caire*, 24, 1911.

(4) K. SETHE. Die Zeitrechnung der alten Aegypter im Verhältnis zu der der andern Völker. *Nachrichten Ges. Wiss. Göttingen*, phil.-hist. Kl., 1919, 287-320; 1920, 28-55.

(5) *Mémoires de l'Institut français du Caire*, 24, 140, 1911 : « La face inférieure du couvercle était occupée par une liste de décans semblable à celle que l'on trouve sur le couvercle des cercueils de MAÏT et de KHITI (p. 117-118 et pl. XXIV-XXV), et sur celui de NAKHITI (puits IV; voir p. 145 et seq.). Comme elle ne fournit aucune variante et est fortement endommagée, nous n'avons pas cru devoir la reproduire ici. » Read HOUNNOU instead of NAKHITI. In a letter from Cairo, dated June 1, 1931, E. CHASSINAT writes : « Le mauvais état de la face inférieure du couvercle ne permet pas de fixer exactement le nombre des colonnes... il est probable que celui-ci devait être pareil. »

(6) See preceding footnote, lines 4 and 6.

Before I pass to a description, from the astronomical point of view, of the MSAHITI, MAIT, KHITI, HOUNNOU, and TEFABI calendars (6a), I shall recall a few elementary facts unknown to or neglected by the Asyutians of the Heracleopolitan period.

2. *Theoretical considerations.*

To fully appeciate the futility of the attempts to construct a diagonal table of the Asyut type, let us consider the following simpler case.

Suppose that an observer located in the latitude of northern Africa choses in a wide belt on both sides of the celestial equator a set of 36 easily identifiable stars in such a way that the difference in right ascension between two consecutive stars of the list is approximately 40 minutes. These stars will cross the meridian of our observer at intervals of about 40 minutes. On a vernal equinox night, the transits of almost one half of the stars of the list will be observable; the next night, each one of these stars will cross the meridian about four minutes earlier (solar time); after ten nights, the time of transit of each decan (ten-day star) will be advanced by about 40 minutes; on an autumnal equinox night, most of the remaining half of the decans of our list will cross the meridian.

A diagonal table of 36 columns recording these transits could be constructed, after the model of Table I, with the names of the decans culminating in the evening listed above the midnight line, and the names of the morning decans listed below that line. Because of the seasonal variation of the length of the night, there will be more transits listed in the winter columns than in the columns corresponding to the summer decads (ten-day weeks); the list will be longest in the column corresponding to the winter solstice, and shortest in the summer solstice decad. The names of the decans culminating at sunset and at sunrise will fall on two sine-shaped curves limiting the table at the top and at the bottom;

(6a) To this group should be added the IDY calendar, of the 24-column type, discovered in Asyut in November, 1908. See my paper, Der Kalender auf dem Sargdeckel des Idy in Tübingen, in *Studien der Bibliothek Warburg*, 19. Heft, Leipzig, TEUBNER, 1932. (Note added to the proofs).

if, for decorative reasons, the frame of the table is made rectangular, non-observable decans culminating, in summer, before sunset and after sunrise must be added, and some of the decans culminating, in winter, after sunset and before sunrise must be omitted.

Such a diagonal table of 36 columns can be used as a calendar, provided that the beginning of the year (consisting of 12 months of 3 decads each, plus 5 days) be made independent from the beginning of the first decad of the first month. The neglected quarter of a day does not matter much, because the differences in right ascension will have sufficiently changed on account of precession to necessitate a revision of our list of 36 decans after the lapse of a millennium.

If culminations are observed at hourly intervals, a set of 24 conveniently chosen stars will suffice; the 24 columns of the diagonal table would correspond to the subdivision of the 360-day year in 24 fortnights, or rather « quinzaines ».

Let us now pass from the simple but artificial case of meridian transits to the more difficult but natural case of heliacal risings (or settings). Suppose that our observer located in the latitude of northern Africa choses a set of 36 easily identifiable stars or compact groups of stars in such a way that they appear (or disappear), in ten-day intervals, at sunrise (or at sunset), on the eastern (or on the western) horizon. The order in wich two consecutive stars of the heliacal-rising list will reach the meridian depends on their right ascension only, and the order in which they will reach the western horizon depends, for a given latitude, both on their right ascension and on their declination. The number of decans whose rising is observable on a given night depends on the season; the length of the seasonal hour (twelfth part of the night) is essentially variable. Let us, in spite of these difficulties, construct a rectangular table of 36 columns and 12 lines. Let us assume that the old year ends—and the new year begins—with the heliacal rising of Sirius. For reasons to be explained below, we shall put the « origin» of our table between the columns 18 and 19, the second (right) semester ending with column 18 and recording, in its lowest case, the first rising of Sirius in the last hour of the night. We now can write, in columns 18 and 19, the names of the decans rising during the twelve short summer hours between sunset and

sunrise. By continuing the diagonals thus begun, we could fill the rhomb between the morning case of column 7 and the evening case of column 30; see Table I. Neglecting, as did the Egyptians,

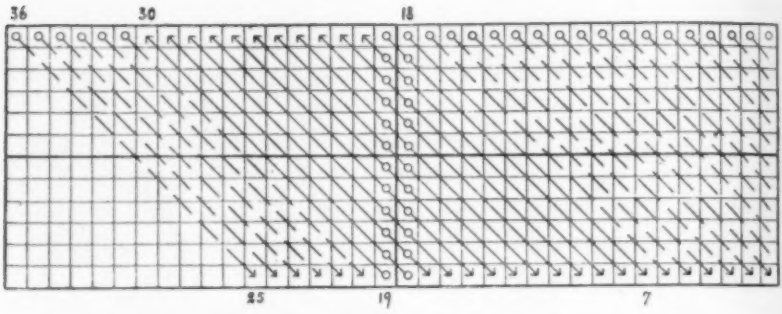

TABLE I. — Scheme of a diagonal table of the 36-column type.

The midnight line—between the rows of the 6th hour (above it) and of the 7th hour (below it)—corresponds to the longitudinal necrological inscriptions of the Asyut calendars. The intersemestrial line—between the second semester (columns 1 to 18) and the first (columns 19 to 36)—corresponds to the transversal pictures of the calendars. The terminal triangle is below the diagonal joining the sunset case of column 36 with the sunrise case of column 25.

for the sake of the diagonal arrangement, the difference between the length of the hours in neighboring columns, we may write, in the upper case of each column, the name of the corresponding decan of our list of 36 stars. Overlooking, for the moment, the astronomical consequences of these compromises, we are able to complete the diagonals of the right half of our table corresponding to the second semester, and the central diagonals of the first semester; the terminal triangle in the left-hand lower corner below the diagonal extending to the sunrise case of column 25 could, of course, be filled with diagonals corresponding to the right-hand upper corner of the table. We now are ready to analyze the astronomical tables of the coffin lids from Asyut.

3. *The Asyut calendars.*

The diagonal tables were drawn on the inside of the inner coffin lids, and oriented in such a way that the mummy lying on

the left side could read them. The topmost row of the table was therefore near the left side of the coffin—the side decorated with the eyes; see Pl. A. The transversal picture—see Pl. C, E, and F—near the central reënforcing cross-piece of the lid was then oriented in such a way that the heads of the divinities were turned toward the head of the mummy, and the hoof of Meskheti toward its feet. The panel between the head-end of the coffin and the central cross-piece was inscribed with the columns of the second semester of the calendar; the other half of the coffin lid had to accomodate both the transversal picture and the columns of the first semester. As the cross-piece was exactly in the center of the lid, and the width of the columns had to be the same for both semesters, the desire to paint the transversal picture on the flat surface of the lid led to the omission of the last columns of the first semester. The MSAHITI calendar forms an exception in this respect : the transversal picture is on the head-end half of the lid, between the last column of the second semester and the central cross-piece; the other half of the lid had, therefore, not only all the space needed for the columns of the first semester, but also space for four additional columns at the foot-end.

The calendarial value of a table of the Asyut type is very limited. The intervals of time between sunset and midnight being unequal in the different decads, the diagonal arrangement of the decans would lead to considerable discrepancies between the results of observation of the sky and of consultation of the table.

4. *The Msahiti and the Tefabi tables.*

These tables belong to the 36-column type characterized by the semestrial division between the 18th and the 19th columns. The six hours of the evening are separated from the six hours of the morning by the usual longitudinal inscription of a necrological nature mentioning the name of the departed and the offerings to astronomical divinities. The intersemestrial transversal picture represents four divinities chosen among those mentioned in the longitudinal inscription : the sky-goddess Nut, Ursa Major, Orion, and Sirius. The place occupied by the images of Sepṭ and Saḥ deserves special attention : the former is crossed by the

diagonals of the Sirius decans 37a and 37, the latter lies in the path of the decans belonging to the Orion complex; this arrangement accounts for the beginning of the year in the middle of the table; Sept and Saḥ of the transversal pictures are illustrations skillfully inserted in the diagonal text of the calendars.

The rubricated column titles give the names of the decads, i. e., indicate the order of the decads within the months and of the months within the seasons; see Table III. Five-pointed stars, the determinatives of the decans, are arranged in columns bordering, on the left, the columns of the calendars.

The TEFABI (7) table—see Pl. D and E—ends with column 32; this lack of space for the end of the first semester results from the traditional position of the transversal picture behind the central cross-piece of the lid; incidentally, Sirius appears in the sunrise case of the last decad of the truncated first semester, in addition to his usual appearance in the sunrise case of the last decad of the second semester. The 32 columns of the TEFABI calendar are the same as the first 32 columns of the MSAHITI table; the slight irregularity of the diagonals in the Orion region is, however, avoided in the TEFABI table; on the other hand, the A-kher (35b) diagonal of the terminal triangle is replaced by an A-ḥer (35a) diagonal ending, in the sunrise case of column 29, with A-kher. Otherwise, the TEFABI calendar is very carefully written, and the state of its preservation is admirable.

The MSAHITI (8) calendar—see Table II—has the distinction of being complete; as I have mentioned above, its peculiar layout made it necessary to add four columns after the end of the first semester. Columns 37 to 39 repeat, with a few errors, the names of the decans as given by the sunset line of the calendar; even the cases usually reserved for the rubricated titles of the decads were filled, at the top of the columns 37 to 39, with names of decans. Column 40 is essentially a repetition of column 36, and ends with the statement that the sum total (of the decans) is 36. The contents of the additional columns 37 to 40 look suspiciously like a memorandum giving the names of the decans

(7) E. CHASSINAT et CH. PALANQUE, *loc. cit.*, p. 193-197. Plates D, E, and F of the present paper are reproduced, from original photographs, with the kind permission of the Cairo Museum.

(8) P. LACAU, *loc. cit.*, p. 105-110.

TABLE II. — Scheme of the diagonal table of MSAHITI.

40	38	36	34	32	30	28	26	24	22	20	18	16	14	12	10	8	6	4	2	
	35[b] 19[a] 6																			
J	20 7	7	6	5[a]	4[a]	3	2[a]	2	37	37[a]	30[b]	33[a]	35[a]	30[a]	30	31	28	23[b]	23[c]	23[a]	22	21	20	19[a]	17	16[a]	15	14[b]	14[a]	13	12	11	10[b]	10[a]	8[a]	7	6
S	33[a] 21 8[a]	J	7			4[a]							35[a]																						7
M	30[b] 22 10[a]	S		7		5[a]							33[a]																						8[a]
35[b]	37[a] 23[a] 10[b]	M			7	6							30[b]																						10[a]
31c?	37 23[b] 11	35[b]				7							37[a]																						10[b]
37[a]	2 23c 12	31[a]				J	7						37																						11
37[b]	2[a] 23[b] 13	37[a]				S		7					2																						12
M?	3 28 14[a]	37[b]				M			7				2[a]						33[a]																13
31?	4[a] 31 14[b]	23[b]				35[b]				7			3						30[b]	33[b]															14[a]
V	5[a] 30 15	31				31[a]					7		4[a]							30[a]	33[b]														14[b]
Total	6 30[a] 16[a]	M				37[a]						7	5[a]								30[a]	33[b]													15
	10[a] 35[a] 17	V	M	31	23[b]	37[b]	37[a]	31[a]	35[b]	M	S	J	7	6	5[a]	4[a]	3	2[a]	2	37	37[a]	30[a]	33[b]	35[a]	30[a]	30	31	28	23[b]	23c	23[a]	22	21	20	19[a]	17	16[a]
		35				30				25				20				15				10				5										

The memorandum occupies columns 37 to 40; columns 37 to 39 repeat the decanologue of the sunset row; column 40, like column 36, gives a list of astronomical divinities heading the diagonals of the terminal triangle. Column 32 is reproduced in full, for the sake of comparison with the final column of the TEFABI table. Column 24 is also reproduced in full, in order to show that it completes the truncated decanologue of the sunset row of a table of the 24-column type obtained by amputation. For the rubricated column titles, see Table III. For the names of the decans, see the TEFABI calendar, Plates D and E. The decans are numbered in accordance with my decanologue of the Empire; see *Isis*, XIV, 317, 1930.

appearing on the upper and on the left margin of a typical calendar; such a memorandum should enable an intelligent craftsman to fill a diagonal table of the 36-column type. The left-margin list was necessary for the traditional filling of the terminal triangle; this left-margin list of column 36 or 40 causes the reappearance, in the terminal triangle, of some of the astronomical divinities mentioned in the longitudinal necrological inscription.

5. *The Khiti and the Hounnou tables.*

These tables belong to the 24-column type characterized by the appearance of the transversal picture between columns 12 and 13. The prototype of the 24-column tables must have been the result of an unsuccessful attempt to improve the tables of the original 36-column type. We have seen that, in addition to the general discrepancy between these tables and the astronomical reality, they contained a terminal triangle constructed on rather obscure, non-calendarial principles. The simplest way of getting rid of the terminal triangle was the reduction of the number of columns to 24. If this reduction were accompanied by the omission of one decan in three, the 12 columns on either side of the transversal picture would correspond to a semester, and the invidual columns would correspond to a fortnight (quinzaine) instead of a decad; the diagonals would, as before, represent qualitatively rather than quantitatively the earlier rising of the decans, the equal upward steps in the neighboring columns corresponding to unequal seasonal hours. Instead of selecting two decans out of the three of each month, the compilers of 24-column calendars were apparently satisfied with copying the first two dozen columns of the larger table. The usual omission of the few last columns near the foot-end of the lid, due to the usual position of the transversal picture behind the central cross-piece, added to the worthlessness of the calendars. If the original 18 columns of the second semester were properly condensed into 12 columns, the lower part of the transversal picture would still remain in the path of the Sirius-Orion decans, i. e., in its proper place; by keeping the transversal picture near the central cross-piece, and by shifting the region of the original column 12 in its neighborhood, the "illustration-in-the-text"

value of the picture is destroyed, and it is degraded to a meaningless traditional decorative feature.

A table of the 24-column type obtained by the simple device of omitting columns 25 to 36 of the original table is worthless as a calendar but it still retains its value as a decanologue : two dozen decans are listed at the top of the table, the third dozen is given by the last column which contains, in its sunrise case, the name of the sunset decan of the first column, thus closing the cycle.

The KHITI (9) table consists of 20 columns, twelve to the right and eight to the left of the transversal picture. They are, in principle, the same as the first 20 columns of the MSAHITI table; there is some confusion in the evening ends of the diagonals of the decans 23a (Ṭepā-khukhu), 23b (Khukhu), 28 (Qeṭ), and 31 (Khau), affecting the left-hand upper corner of the table. The usual star border, on the left side of each column, is present. The rubricated column titles have been omitted—they are useless and confusing in a table of the 24-column type.

The HOUNNOU (10) table—see Pl. B and C—consists of 21 columns, twelve to the right and nine to the left of the transversal picture. As in the case of the KHITI table, they correspond to the first 20 columns of the MSAHITI calendar. The additional column of the HOUNNOU table is due to the fact that the diagonal of the decan 8a (Ushbekat)—common to the MSAHITI, MAIT, KHITI, and TEFABI calendars—is replaced by the two diagonals of the decans 8 (Usht) and 9 (Bekat); this feature of the HOUNNOU calendar deserves closer attention.

The diagonals of the decans 22 (Kher-khept-sert) and 23a (Ṭepā-khukhu) are interchanged on the left side of the transversal picture. In addition to the usual borders of five-pointed stars, the table is provided with rubricated column titles.

6. *The Mait and the pseudo-Nakhiti tables.*

The MAIT (11) table is an excellent example of the work done

(9) E. CHASSINAT et CH. PALANQUE, *loc. cit.*, p. 127-128. A reproduction of CH. PALANQUE's photographs of the KHITI coffin lid, *ibid.*, pl. XXIV and XXV.

(10) *Ibid.*, p. 145-148. Plate A of the present paper is a reproduction, from a photostat copy, of pl. XXVIII, by courtesy of E. CHASSINAT. Plates B and C are reproduced here, from original photographs, with the kind permission of the Musée du Louvre.

(11) E. CHASSINAT et CH. PALANQUE, *loc. cit.*, p. 116-119.

by a decorator to whom the astronomical meaning of the table was completely lost. He was quite satisfied with copying the first 16 columns of the traditional table, and he inserted the transversal picture between the 9th and the 10th columns, because he knew that it had to appear near the central cross-piece of the lid. As could be expected, there are a few lapsus calami; the usual borders of five-pointed stars, and even the rubricated column titles are present. The MAIT table belongs to the degenerated 24-column type.

The pseudo-NAKHITI (12) table is in such a poor state of preservation that it is difficult to classify it; apparently, it belongs to the 24-column type; it ought to be possible to estimate the height of the transversal picture and the width of the columns, and thus to determine the number of columns on either side of the transversal picture. A closer examination of the pseudo-NAKHITI table could conceivably reveal the separate diagonals of Usht and of Bekat which are, among the Asyut calendars, a unique feature of the HOUNNOU table.

7. *The rubricated column titles.*

We have seen that the calendars of the 36-column type were constructed in such a way that the heliacal rising of Sirius, recorded in the sunrise case of column 18, marked the end of the second semester (on the right of the transversal picture) and the beginning of the first semester (on the left).

The rubricated column titles completely disregard this arrangement. They do not begin, in column 18, with the name of the decad which happened to contain, in the year of the manufacture of the coffin, the heliacal rising of Sirius; they pay no attention to the number of columns—32 or 21 or 16 ; they completely disregard the position of the transversal picture—after the 18th or the 12th or even the 9th column. Table III shows clearly that all preserved lists of rubricated column titles reflect the very naïve hypothesis that column 1 corresponds to the first decad of Thoth; thus, the rubricated title of column 9 is always, "last decad of

(12) *Ibid.*, p. 140. See also footnote (5) of the present paper.

TABLE III. — Rubricated column titles.

Season	Month	Decad	Column			
			MSAHITI	TEFABI	HOUNNOU	MAIT
Shat	1st	first	(1)	1	1	—
		middle	—	2	2	—
		last	(3)	3	3	—
	2nd	first	(4)	4	(4)	—
		middle	(5)	5	—	—
		last	(6)	6	(6)	—
	3rd	first	7	7	7	7
		middle	8	8	8	8
		last	9	9	9	9
	4th	first	10	10	—	10
		middle	11	11	—	11
		last	12	12	—	(12)
Peret	1st	first	13	13	13	13
		middle	14	14	14	14
		last	15	15	15	15
	2nd	first	16	16	—	16
		middle	17	17	—	
		last	18	18	(18)	
	3rd	first	—	19	19	
		middle	—	20	(20)	
		last	(21)	21	21	
	4th	first	(22)	22		
		middle	(23)	23		
		last	(24)	24		
Shomu	1st	first	25	25		
		middle	26	26		
		last	27	27		
	2nd	first	28	28		
		middle	29	29		
		last	30	30		
	3rd	first	31	31		
		middle	32	—		
		last	33			
	4th	first	34			
		middle	35			
		last	36			

the 3rd month of Shat", in spite of the fact that the decans of column 9 of HOUNNOU are listed in column 8 of the other calendars, and irrespective of the presence of the transversal— theoretically, intersemestrial—picture between columns 9 and 10 of MAIT.

The dating of the MSAHITI (13) and TEFABI calendars—or, rather, of their prototype—by the title of column 18, "last decad of the 2nd month of Peret", is therefore illusive : even if the last decad of Mekheir marked, in the middle of the last century of the third millennium, the beginning of the Sothic year, the rubricated title of the 18th column of an Asyut calendar is merely the 18th item of a list of names of decads beginning with the first decad of the 1st month od Shat. The average modern counterpart of an Asyut craftsman would think of January 1st as the natural beginning of a calendarial table giving, without dates, the time of moonrise for every day of a year—especially if the second part of the calendar preceded, in the table, its first part, and if the beginning of this first part corresponded, e. g., to the first Sunday after the full moon following or coinciding with the vernal equinox. The Asyut coffin decorators were not astronomers. The craftsman who wrote the KHITI calendar found it expedient to omit the meaningless column titles, balancing, incidentally, the number of lines above and below the longitudinal inscription.

In Table III, parentheses indicate not only partly illegible names but also well preserved titles of middle or last decads of partly illegible months or seasons. Thus, the titles of the columns 6 of MSAHITI and of HOUNNOU certainly refer to the last decad, but the titles of columns 4 of these tables are so damaged that the number of the month (on the MSAHITI table) and the name of the season (on both tables) remain under suspicion—theoretically, at least. In the case of the HOUNNOU coffin lid, I have used, in addition to the original photographs reprocuded in Pl. B and C, the published CHASSINAT transcription; thus, the title of column 7 (on which the column titles 8 and 9 depend for their interpretation) is now completely lost, yet it was clearly legible a quarter of a century ago.

(13) E. ZINNER. Die Geschichte der Sternkunde, p. 19, 1931. « Aus der Lage dieses Trennungsbandes zu den Monaten des beweglichen Jahres lässt sich das Alter der betreffenden Liste berechnen, und es ergibt sich für Msahiti zu 2047 v. Chr. »

8. *The Asyut calendars and the ceilings of the XVIIIth and XIXth dynasties.*

We have seen that the sunset line and the final column of the Asyut tables play an important role. In the case of the complete 36-column tables, the sunset line gives the list of stars appearing, at sunset, on the eastern horizon; the last column supplies the material for the non-calendarial terminal triangle. In the case of the complete 24-column tables, the list of decans runs through the sunset line and down the last column which ends with a sunrise case corresponding to the sunset case of the first column; thus, a memorandum or guiding fringe for the construction of a 24-column diagonal table would consist, by analogy with the memorandum attached to the MSAHITI calendar, simply of a list of decans.

I have pointed out that the calendarial value of the diagonal tables of the 36-column type was rather limited, and that the 24-column tables obtained by amputation—not by contraction—were worthless as calendars. It is therefore not surprising that the diagonal tables were finally abandoned. They exerted, however, an influence on the ceiling decorations of the tombs of the New Kingdom—an influence which deserves to be analyzed.

The coffin lids from Asyut and the tomb ceilings of the XVIIIth and XIXth dynasties are divided in two longitudinal panels by an axial necrological inscription. The transversal picture in the center of the lids accounts for the Ursa Major group in the center of the northern panel, and for the Orion-Sirius group in the center of the southern panel of the ceilings. The unfinished monthly star charts on the northern panel of the SENMUT ceiling (14) seem to reflect an attempt to correlate by observation and extrapolation the hours of the rising of the decans with the 24 hours of the first day of the first decad of each month in a more satisfactory manner than the diagonal tables ever permitted : 12 circles subdivided into 24 hours were better suited to represent monthly changes in the time of rising of the decans than were diagonal tables which disregarded the seasonal changes of the length of the 12 hours of the night.

(14) See *Isis*, XIV, 312, 1930.

9. *The sunset row of the calendars and the decanologues of the ceilings.*

There is a connection between the upper line (sunset cases, of course, not rubricated column titles) of the Asyut calendars and the upper line of a southern panel of the tomb ceilings. The decanologues of the XVIIIth and the XIXth dynasties begin with Ṭepā-kenmut, a decan not mentioned on our diagonal tables, and end with the former leader of the decans, Sepṭ; this 37th decan occupies the 29th column on the Senmut ceiling, and the 28th column in the Ramesseum. Now, in the diagonal tables of the MSAHITI-TEFABI type, the sunset case of the 29th column is occupied by the decan Sepṭ, and the sunset case of the 28th column by his precursor, Ṭepā-sepṭ. The desire of the ceiling decorators to accommodate the full decanologue of their epoch in 29 or 28 columns (15) necessarily led to the allotment of some of the columns to more than one decan. Thus, the decans corresponding to those of the sunset cases following Sepṭ, and to those of the sunset cases preceding Khentet-ḥert, were grouped by pairs in the columns at the right of the SENMUT and the Ramesseum decanologues in such a way that the Khentet group could be reinstalled at the head of columns 6, 7, and 8—their traditional place in the sunset row of the diagonal tables. Similarly, the presence of the decans of the Semṭ, Sert, and Khukhu groups in the SENMUT-Ramesseum columns numbered 14 to 19 is due to the traditional place occupied by these decans in the sunset cases of the columns 14 to 20 of the calendars.

Decan 18, Ṭepā-semṭ, is a good illustration of the connection between the sunset row of the diagonal tables and the decanologues of the ceilings. This decan is not mentioned on the coffin lids, where Semṭ-sert (sunrise case 14) directly follows Kenmu

(15) In the case of the SETI ceiling, the number of columns of the decanologue is even less, namely 23. This decanologue follows another tradition, as I have pointed out in *Isis*, XIV, 320, 1930. It is probable that this tradition goes back to the sunset row of another type of diagonal calendar, e.g., of the 24-column type obtained by « three-to-two » contraction, not by amputation. In such a table, Sirius would appear in the sunrise case of column 12, at the end of the second semester; the Sirius diagonal would lead to the senset case of column 23; on the SETI ceiling, Sirius appears in the top case of column 23.

(sunrise case 13). The natural place for decan 18, Ṭepā-semṭ, in the decanologues of the ceilings, is obviously between decan 17, Kenmu, and decan 19, Semṭ. On the SENMUT ceiling, the zig-zag stars of Ṭepā-semṭ are inserted, in column 13, between the constellation of the Boat and the constellation of the Ram. On the SETI ceiling, Kenmu and Semṭ occupy the traditional positions at the top of columns 13 and 14, held by these decans since the third millennium, and the name of the intruding Ṭepā-semṭ is added, awkwardly, disregarding the line separating the names of the decans from their determinative stars. In the Ramesseum, column 13 is reserved for the name of a decan following Kenmu and preceding Semṭ, but the name of the intruder has been removed. The trouble caused on the ceilings of the XVIIIth and XIXth dynasties by Ṭepā-semṭ is due to the rigid traditions inherited from the sunset row of the diagonal tables of the third millennium.

Although confusion reigns, as a rule, in the Orion region of all decanologues, the rising of Ārt recorded in the sunset case 23, and the rising of its lower part, Ārt-khert, recorded in the sunset case 27, are responsible, in all probability, not only for the mentioning of decan 30, Ārt, in column 23 of the Ramesseum but also for his rather unexpected appearance in column 27 of the SENMUT decanologue.

These examples will suffice to show the connection between the sunset row of the calendars and the decanologues of the ceilings. A similar connection exists between the last column of a complete calendar of the 36-column type and the meta-Sothic lists of the southern panels of the ceilings.

The relation of the Asyut calendars to the Ramesseum and Medinet Habu ceilings and to the alabaster clepsydra in Cairo will be discussed in a forthcoming paper.

10. *The terminal triangle and the meta-Sothic lists.*

We have seen that the necrological longitudinal inscriptions mention divinities relegated to the terminal triangle of the tables of the 36-column type. Some of the divinities of the terminal triangle are mere decans belonging to the groups of Orion, Sirius, Khau, and Khukhu. Whether the Semṭ-res and the Semṭ-mehti

divinities are related to the decans of the Semṭ group, or represent planets wandering in the southern and the northern sky, deserves a closer scrutiny.

The Orion and the Sirius group is followed, on the southern panels of the ceilings, by a rather heterogeneous group of planets and of retired decans. There is a certain analogy between the last column of a complete diagonal table, or the last column of a memorandum of the MSAHITI type, on the one hand, and the meta-Sothic group of a southern panel, on the other. If this analogy holds, the Semṭ-Semṭ-Nuter trio of the upper diagonals of the terminal triangle, which also follows the Nut-Saḥ or Nut-Saḥ-Sepṭ or Nut-Saḥ-Nut group of the longitudinal inscriptions, could represent the Jupiter-Saturn-Mars trio which follows the Saḥ-Sepṭ group on the southern panels of the ceilings.

The traditional place for the planet Venus, on the southern panels, was in the last column; in the sunrise case of the 36th column of a complete diagonal calendar there is an otherwise unknown astronomical divinity, probably Venus. The neighboring short diagonal of the Nuter divinity was probably intended to represent Mercury, not Mars, if the analogy with the penultimate column of southern panels holds.

The terminal triangle was a resting place for discarded decans, like the meta-Sothic region of the southern panels of the next millennium. The decans A-kher, Khafu, and Am-khet-sepṭ of the terminal triangle have, apparently, outlived their usefulness; their names are not mentioned in the calendarial part of the diagonal tables. The names of the astronomical divinities mentioned in the necrological longitudinal inscriptions deserve to be studied.

11. *The decanologues of Asyut.*

A comparative study of the decanologues from Asyut on the one hand, and of the list of decans of the New Kingdom, on the other, remains to be made. A few remarks on the differences between the decanologues of the diagonal tables and of the southern panels of the XVIIIth and XIXth dynasties seem, however, appropriate.

Due to precession, the appearance of constellations on the eastern horizon undergoes gradual changes. After the lapse of

a millennium—or even of several centuries—it may become appropriate to merge two neighboring decans into one, or to split a decan into two, to omit or to introduce detailed descriptions of parts of a constellation; it may even become advisable to chose a new constellation whose time of rising fits better into the changed schedule of the neighboring decans. The difference in latitude—and even the outline of the eastern horizon—must not be neglected, if decanologues developed in different localities have to be compared.

To illustrate these changes, I shall give a few examples under the following headings : (A) Obsolete decans of the terminal triangles; (B) Active Asyut decans in the meta-Sothic lists of the ceilings; (C) Coalescence, splitting, and inversion of decans; (D) Asyut decans not found of the ceilings of the XVIIIth and XIXth dynasties; (E) Decans of the XVIIIth and XIXth dynasties not traceable to the Asyut tables.

(A) I have mentioned, in section 10, the decans A-kher, Khafu, and Am-khet-sept, of the terminal triangles; their names do not occur in the calendarial part of the diagonal tables, and are not to be found on the ceilings of the Empire. They were active decans in the first half of the third millennium—or earlier! In Table II, their respective numbers are 35b, 31a, and 37b.

(B) The decans Apeset, or Abshes, and Sebshes, mentioned in the meta-Sothic lists of the XVIIIth and XIXth dynasties, saw active service, in the IXth and Xth dynasties, and again in the Graeco-Roman period. In Table II, I have given them the numbers 10a and 10b.

(C) The Kenmut group offers a good example of the carving of new decans out of old constellations : the diagonal table mentions Kenmut, Sasa-kenmut, and Kher-khept-kenmut; the southern panels of the ceilings omit Sasa-kenmut but add Ṭepā-kenmut at the head of the group, inverting—in the SENMUT-Ramesseum tradition—the order of Kher-khept-kenmut and of Kenmut. As could be expected, the Orion-Sirius region is especially rich in similar examples of coalescence, splitting, and inversion of decans.

(D) The names of some of the decans mentioned in the calendarial part of the diagonal tables have shared the fate—oblivion—of the obsolete decans and of planetary names of the terminal

triangle. Most conspicuous among them are the names of the constellations of the Two Fishes (decan 14b) and of the Two Squatting Pairs (decan 16a); these group of stars belong to the great constellation of the Boat, and the horizontal row of four stars (SENMUT ceiling) may possibly represent the heads of four squatting sailors or fishermen.

(E) The names of some of the decans mentioned on the southern panels have no direct connection with the diagonal tables. Thus, decan 24, Baba, and decans 25 to 27, the second Khent group, cannot be traced to the Asyut tables.

TABLE IV. — Diagonal tables and southern panels.

Group	Decans
(A) Obsolete, terminal triangles	31a, 35b, 37b
(B) Obsolete, meta-Sothic lists	10a, 10b
(C) Coalescence, splitting, inversion	1, 2a; 10; 18, 19, 19a; 23c; 29; 30a, 30b; 33a, 33b; 35a, 35b; 37a
(D) Calendars only	4a, 5a, 8a, 14a, 14b, 16a
(E) Ceiling decanologues only	4, 5, 14, 24, 25, 26, 27, 32a, 32b, 34, 36a, 36b

The preceding table shows, in spite of its preliminary character and its limited scope, that a comparative study of the decanologues of the third millennium and of the New Kingdom is long overdue.

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On the Relation of Mahāvīra to Śrīdhara

Introduction

In a very old and worn manuscript of MAHĀVĪRA's Gaṇita-sāra-saṁgraha, belonging to the collection of Dr. BHAU DAJI, SANKARA BALKRISNA DIKSHIT discovered upwards of thirty years ago a reference to ŚRĪDHARA by name (1), together with a quotation from one of his works. The quoted passage does not occur in any of the known manuscripts of ŚRĪDHARA's Triśatikā (2), and its metre which is the Anuṣṭup, is different from that of the Triśatikā, which is the Āryā. So DIKSHIT concluded that the passage in question must be either from the larger arithmetical treatise (Pāṭi-gaṇita) of ŚRĪDHARA or from his Bīja-gaṇita. From ŚRĪDHARA's own admission, as well as from references by later writers, there is no doubt as to these works having been written although no copies are now extant. SUDHAKARA DVIVEDI, while correcting a little inadvertence of DIKSHIT as regards the quotation in question, accepted his views in general. He also pointed out that a certain other quotation from the Pāṭi-gaṇita of ŚRĪDHARA by SŪRYADĀSA (1538) (3) in his commentary on the Līlāvatī of BHĀSKARĀCĀRYA II (1150) is in the same Anuṣṭup metre. (4) G. R. KAYE (5), on the other hand, has challenged the authenticity

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(1) SANKARA BALKRISNA DIKSHIT, History of Indian Astronomy, Poona, 1896, p. 229-30.

(2) *Ibid.*, p. 312.

(3) All dates refer to the Christian era.

(4) Triśatikā, edited by SUDHAKARA DVIVEDI, Benares, 1899, Preface. The main rules of this treatise, without the illustrative examples and statements, have been translated into English by N. RAMANUJACHARIAR and published with notes and comments by G. R. KAYE (« The Triśatikā of ŚRĪDHARĀCĀRYA ») in the *Bibliotheca Mathematica*, vol. XIII (3), 1912/13, p. 203.

(5) *Bibl. Math.*, vol. XIII (3), p. 205.

of the quotation on three grounds : (i) the inadvertence in the quotation as it appears in DIKSHIT's book, (ii) its absence from the Triśatikā as available at present, and (iii) its absence from the Gaṇita-sāra-saṁgraha as edited by RANGACARYA. For these reasons KAYE discarded DIKSHIT's hypothesis that ŚRĪDHARA was anterior to MAHĀVĪRA. The third objection of KAYE is not tenable since the quotation in question actually occurs in RANGACARYA's edition of the Gaṇita-sāra-saṁgraha. (6) But in this edition, it should be noted, it is not stated as having been taken from ŚRĪDHARA, as it is in the manuscript consulted by DIKSHIT. Indeed there is no mention of ŚRĪDHARA or of any other writer by name in the printed edition of the Gaṇita-sāra-saṁgraha. The other objections of KAYE do not seem to be at all material, for a distinguished algebraist like ŚRĪDHARA, whose authority has been freely admitted even by the celebrated BHĀSKARA II (7), was certainly acquainted with the rule of signs and must have stated it in his algebra.

It now appears on careful scrutiny and comparison that the relation of MAHĀVĪRĀCĀRYA to ŚRĪDHARĀCĀRYA was much closer than has hitherto been suspected. MAHĀVĪRA was not only well acquainted with, but was also greatly influenced by the writings of ŚRĪDHARA. I shall support this idea by referring chiefly to (i) the range of topics discussed by both these writers, (ii) a certain peculiarity in their method of treatment of various topics, and (iii) the similarity of certain illustrative examples found in their available works, basing my study on SUDHAKARA DVIVEDI's edition of the Triśatikā of ŚRĪDHARĀCĀRYA and RANGACARYA's edition of the Gaṇita-sāra-saṁgraha of MAHĀVĪRĀCĀRYA. Further, in comparing the works of these two authors with those of other Hindu writers, I have taken note of the works of ĀRYABHAṬA I (499), BRAHMAGUPTA (628), ĀRYABHAṬA II (c. 950), ŚRĪPATI (1039) and BHĀSKARĀCĀRYA II (born 1114), but not of those Hindu mathematicians who came after BHĀSKARA.

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(6) Gaṇita-sāra-saṁgraha, edited and translated into English by M. RANGACARYA Madras, 1912, Ch. 1, verse 52.

(7) H. T. COLEBROOKE, Algebra with arithmetic and mensuration from the Sanscrit of BRAHMEGUPTA and BHĀSCARA, London, 1817, p. 275; hereafter referred to as COLEBROOKE, Hindu Algebra.

Range of topics

The range of topics discussed in the Gaṇita-sāra-saṁgraha is practically the same as that in the Triśatikā with some slight variations which will be noted and accounted for later on. The arrangements of topics are almost parallel. But what is of still greater importance for our present purpose is the fact that there are certain topics which have been treated in these two works only, but are not found elsewhere. For instance, both ŚRĪDHARA and MAHĀVĪRA make a separate sub-section for the subject dealing with « the sale of animals. » (8) Reference to it is also made by ŚRĪPATI. (9) BHĀSKARA II gives a single example of it, but only as an application of the inverse rule of three, whereas BRAHMAGUPTA and ĀRYABHAṬA II give none.

The rule for finding the area of a segment of a circle is given by ŚRĪDHARA, MAHĀVĪRA, and ĀRYABHAṬA II, but not by ĀRYABHAṬA I, BRAHMAGUPTA, or BHĀSKARA. (10) The formulas for the chord, height, and diameter of the circle occur, however, in nearly all the Hindu arithmteical treatises. The earliest writer to give them, expressly, so far as is known, was UMĀSVĀTI (c. 150). (11) If c denotes the chord and h the height of the segment, then, according to ŚRĪDHARA, (12)

$$\text{Area} = \sqrt{\tfrac{10}{9}} \cdot h\ (c+h)/2;$$

according to MAHĀVĪRA, (13)

$$\text{Gross Area} = h\ (c + h)/2,$$

$$\text{Neat Area} = \tfrac{1}{4}\ ch\ \sqrt{10}\ ;$$

and according to ĀRYABHAṬA II, (14)

$$\text{Gross Area} = \sqrt{\tfrac{10}{9}} \cdot h\ (c + h)/2,$$

$$\text{Neat Area} = \tfrac{22}{21}\ h\ (c + h)/2.$$

(8) Triśatika, R. 32, Ex. 56; Gaṇita-sāra-saṁgraha, v. 39-41.

(9) Siddhānta-Śekhara, XIII, 17.

(10) In his introduction to the Gaṇita-sāra-saṁgraha, p. XXII, Professor D. E. SMITH errs in stating that the area of a segment of a circle is treated by both BRAHMAGUPTA and BHĀSKARA.

(11) Tattvārthādhigama-sūtra of UMĀSVĀTI, edited with UMĀSVĀTI's commentary by K. P. MODY, Calcutta, III, 11 (Com).

(12) Triśatikā, R. 47, Ex. 86.

(13) Gaṇita-sāra-saṁgraha, VII, 43, 44, 70½-72½.

(14) Mahāsiddhānta, edited by SUDHAKARA DVIVEDI, Benares, 1910, XV, 89, 93.

Obviously the first formula of MAHĀVĪRA is derived from ŚRĪDHARA's formula by taking 10/9 equal to 1, which is very nearly accurate. (15) His other expression seems to have been obtained, as has been already pointed out by RANGACARYA, by analogy with the expression for the area of the semicircle, viz. $\frac{1}{2}\pi r^2$, which can be written in the form, diameter $\times \frac{1}{4}$ height $\times \sqrt{10}$. The second expression of ĀRYABHAṬA II is clearly obtained from ŚRĪDHARA by substituting 22/7 as the value of π in the place of $\sqrt{10}$.

There are certain other kinds of plane figures such as of the shape of a yava (barley corn), muraja or mṛdanga (drum), gajadanta (elephant's tusk), bālendu (crescent moon), nemi (felloe), vajra (thunderbolt), etc., which have been treated by ŚRĪDHARA, MAHĀVĪRA, and ĀRYABHAṬA II, but not by other writers. It should also be noted that in respect to these figures, agreement between ŚRĪDHARA and ĀRYABHAṬA II is more pronounced than that between ŚRĪDHARA and MAHĀVĪRA.

There is similarity between ŚRĪDHARA and MAHĀVĪRA in another matter. Both have given considerably more space to the treatment of fractions than that of any other topic. Thus, in the Gaṇita-sāra-saṁgraha, out of a total of nine chapters, two (ch. III and IV) are devoted to the treatment of fractions; one chapter deals with the eight fundamental operations with fractions, and is followed by another containing the miscellaneous problems on fractions. Similarly, in the Triśatikā, nine rules out of a total of 65, and 21 examples out of 107, are devoted to fractions. In this book also there is a collection of examples after the preliminary statement of the fundamental operations with fractions. Further, among the examples in the two books some are similar and one is the same *(vide infra)*. The next topic which has received great attention is the Rule of Three. Such a preferential treatment of fractions or of the Rule of Three is not found in any other available Hindu arithmetical treatise.

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(15) KAYE seems to have implied that MAHĀVĪRA's formula was obtained from the Chinese source. (Query. Editor). (Indian Mathematics, Calcutta, 1915, p. 39). Any such presumption is futile in view of ŚRĪDHARA's result.

Method of treatment.

In their method of treatment ŚRĪDHARA and MAHĀVĪRA agree in many respects. What is still more noteworthy is the fact that there are certain peculiarities in their works which are not found elsewhere. We shall corroborate this statement by the following instances :

1. Both ŚRĪDHARA and MAHĀVĪRA have combined the two fundamental operations of Addition (saṁkalita) and Subtraction (vyavakalita or vyutkalita) with the summation of simple series. The whole subject of summation is again treated separately in the latter part of the works as śreḍī-vyavahāra or śreḍī-vaddha-saṁkalita. This mixing up of ideas does not occur in any other Hindu treatise on arithmetic. (16)

2. According to ŚRĪDHARA, MAHĀVĪRA, and ŚRĪPATI, the operation of multiplication may begin from the first or the last figure (anuloma or viloma process). (17)

According to BHĀSKARA II and ĀRYABHAṬA II it must begin from « the last figure », that is, by the viloma process. (18) BRAHMAGUPTA is silent on this point. Again both ŚRĪDHARA and MAHĀVĪRA taught the same methods of multiplication, one of which, the tastha, has not been mentioned in any other known earlier work. We, however, have it on the authority of PṚTHUDAKASVMĀĪ (860) that it was stated by another Hindu mathematician, SKANDASENA by name.

3. ŚRĪDHARA and MAHĀVĪRA, as well as ŚRĪPATI, remark that the operation of division must be by the viloma process only. Such a remark has not been made by any other writer. It is, in fact, unnecessary in their case as they have nowhere sanctioned the anuloma process.

4. ŚRĪDHARA as well as MAHĀVĪRA has indicated the same four methods of squaring a number : (19)

(16) It was, in fact, no unconscious confusion. For ŚRĪDHARA states that « the rules from another section have been included in the section on addition simply to give one who has acquired knowledge of adding different numbers an idea of such a process of summation and thus to lighten his task. »

(17) Triśatikā, R. 5; Gaṇita-sāra-saṁgraha, II, 1; Śiddhānta-śexhara, XIII, 2.

(18) Mahāsiddhānta, XV. 3; Līlāvatī (COLEBROOKE, Hindu Algebra, p. 5).

(19) Triśatikā, R. 10, 11; Gaṇita-sāra-saṁgraha, II, 29-31.

(I) $a^2 = a \times a$

(II) $a^2 = (a + N)(a - N) + N^2$

where N is any arbitrary number suitably chosen.

(III) $(a + b + c + \ldots)^2 = a^2 + b^2 + c^2 + 2a(b + c + \ldots) + 2b(c + \ldots) + \ldots$

(IV) $a^2 = 1 + 3 + 5 + \ldots$ to a terms.

This last method of squaring is not found elsewhere, although the Greeks knew that the sum of the first n odd numbers is n^2.

5. ŚRĪDHARA as well as MAHĀVĪRA recognizes six processes of reduction of fractions (jāti), viz., Bhāga-jāti (« simple fraction »), Prabhāga-jāti (« fraction of fractions »), Bhāgānubandha-jāti (« fractions in association »), Bhāgāpavāha-jāti (« fractions in dissociation »), Bhāga-bhāga-jāti (« Complex fractions ») and Bhāga-mātā-jāti (« mother-fractions »). The Bhāga-mātā which consists, indeed, of a combination of the two or more of the other varieties of fractions is not found separately in other available Hindu arithmetical treatises. According to BRAHMAGUPTA, for example, the number of rules for the reduction of fractions is five, and BHĀSKARA II omits the Bhāga-bhāga-jāti also. We learn from PṚTHUDAKASVĀMĪ that the Bhāga-mātā was considered by SKANDASENA. (20)

6. ŚRĪDHARA and MAHĀVĪRA mention four varieties of the Rule of Three : the simple, double, treble, and quadruple. BRAHMAGUPTA and BHĀSKARA II added a fifth variety, the multiple Rule of Three, consisting of eleven terms.

Similar illustrations

There are certain illustrative examples in the Triśatikā and the Gaṇita-sāra-saṁgraha which are similar and which naturally attract attention. One of them is the same in the two works.(21)

« Out of a collection of excellent bees, 1/6 took delight in pāṭalī trees, 1/3 in kadamba trees, 1/4 in mangon trees, 1/5 in a campaka tree with blossoms fully opened, 1/30 in a collection of full-blown lotuses, opened by the rays of the sun; and (finally) a single intoxicated bee was circling in the sky. What is the total number of bees? » N.P. This problem appears in both the works with a

(20) COLEBROOKE, Hindu Algebra, p. 283.

(21) Triśatikā, Ex. 28; Gaṇita-sāra-saṁgraha, IV, 6.

very slight verbal alteration at one or two places which is doubtless due to an error of the copyists, and not to the original writers. Therefore this problem has either been taken by the one from the other, or else both writers took it from a third source. As long as that third source, if there be really one, is not discovered, however, it will perhaps not be unnatural to believe in the first alternative.

This belief as to possible sources is strongly supported by the similarity of certain other examples in the sections on fractions and the Rule of Three. One such is about a lady with a necklace. In the Triśatikā it runs as follows : (22)

« A lady with a necklace of pearls breaks her necklace in an amorous quarrel. The third part of it falls on the ground, the fifth part is seen lying on the bed, one-sixth is taken up by the fair haired damsel, one-tenth is picked up by her beloved, and six pearls are seen (as remaining) on the string. Say, oh mathematician, of how many pearls the necklace was composed? »

The same problem reappears in RAMKRISNADEVA's commentary, the Manoraṇjanī, on the Līlāvatī of BHĀSKARA II. (23) The language of the Gaṇita-sāra-saṁgraha is more romantic and full of poetic licence, and there are also some slight differences in detail (24). Among the other similar examples are : (1) the post in the river (25), (2) the lame man taking a long walk (26), and (3) a worm crawling a great distance. (27)

An imperfect rule relating to the gnomon, together with two illustrative examples, is given in the Triśatikā of ŚRĪDHARA. With certain verbal but unimportant changes it also appears in the Gaṇita-sāra-saṁgraha of MAHĀVĪRA (28), and still earlier in the work of BRAHMAGUPTA.

Points of Difference

We have thus far noted only certain similarities between the

(22) Triśatikā, Ex. 26.
(23) COLEBROOKE, Hindu Algebra, p. 25.
(24) Gaṇita-sāra-saṁgraha, IV, 17-22.
(25) Triśatikā, Ex. 23, 24, 25, 29; Gaṇita-sāra-saṁgraha, IV, 5, 70.
(26) Triśatikā, Ex. 35; Gaṇita-sāra-saṁgraha, V, 4.
(27) Triśatikā, Ex. 36; Gaṇita-sāra-saṁgraha, V, 5.
(28) Triśatikā, R. 65, Ex. 106, 107; Gaṇita-sāra-saṁgraha, IX, 8½-11½.

Triśatikā and the Gaṇita-sāra-saṁgraha, although various others could easily be cited. (29) We now propose to point out some notable differences between them. The first one that will attract the attention of a reader is the difference in the arrangement of the fundamental arithmetical operations. MAHĀVĪRA has, very strangely, placed the operations of addition and subtraction after all the rest. The failure to appreciate the true significance of ŚRĪDHARA's explanation of his own extraordinary departure, which has been noticed before, is probably responsible for it. The Triśatikā does not contain the treatment of geometric progressions, the ellipse, permutations and combinations, or indeterminate analysis, all of which together with certain other matters find places in the Gaṇita-sāra-saṁgraha. The last mentioned subject, particularly the solution of the rational quadrilateral, received special attention in the work of MAHĀVĪRA. (30) The latter also gives a copious number of examples to illustrate each rule, while ŚRĪDHARA gives only a few. Thus the differences between the Triśatikā and the Gaṇita-sāra-saṁgraha are not very formidable inasmuch as they consist in fuller descriptions and further extensions in the latter work, but not in contradictions. These are probably due to MAHĀVĪRA's treatise being more extensive,—the largest of all the known Hindu arithmetical works,—whereas ŚRĪDHARA's is only a small book, a mere compendium of a larger treatise. Considering the problem as a whole, there is nothing in these two books to preclude the possibility of their being related to each other, possibly through a common source; on the other hand there are many things to strongly suggest such a probability.

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(29) For instance, the section of fractions in both the works bears the title Kalā-savarṇa, while in other works it is called the Bhinna (or « the broken quantity »). The use of the first term is peculiar. The word kalā, when used with a numerical significance, ordinarily denotes « the sixteenth part », or occasionally « sixteen ». In the above the term kalā means « parts resembling one-sixteenth », hence « fractions in general. » Again MAHĀVĪRA as well as ŚRĪDHARA carried the Hindu decimal numeration beyond the eighteen places usually found in the treatises on arithmetic. Compare COLEBROOKE, Hindu Algebra, p. 4.

(30) Compare the writer's article, « On MAHĀVĪRA's solution of rational triangles and quadrilaterals », *Bulletin of the Calcutta Math. Soc.*, vol. 20 (1928-1929), pp. 267-294.

Conclusion

From what has been stated above there can be little doubt that either ŚRĪDHARA or MAHĀVĪRA was greatly influenced, directly or indirectly, by the writings of the other. But these facts do not help us much to ascertain the priority of either of them over the other. If, however, the passage in SANKARA BALKRISNA DIKSHIT's copy of the manuscript of the Gaṇita-sāra-samgraha which contains reference to ŚRĪDHARA be truly authentic, then ŚRĪDHARA surely lived before MAHĀVĪRA. There is another fact which will throw some light on this matter. ŚRĪDHARA was undoubtedly the most distinguished mathematician of his time. His reputation seems to have spread far and wide, more than that of any other early Hindu mathematician, not excepting the great BHĀSKARĀCĀRYA II (born 1114). (31) Even in the middle of the thirteenth century the ideal of a Hindu mathematician was to be a ŚRĪDHARA (32). Such a writer can hardly be believed to have been a mere borrower. On the other hand, it has been expressly stated by MĀHĀVĪRA that his treatise is a « collection » (saṁgraha) from the works of previous writers.

(University of Calcutta). BIBHUTIBHUSAN DATTA.

(31) In a certain manuscript of the Triśatikā, SUDHAKARA DVIVEDI found a colophon purporting to state that throughout the length and breadth of India, there was not a greater mathematician than ŚRĪDHARA. Cf. Triśatikā, Preface.

(32) *Vide* Kavi-kalpa-latā-vrtti of AMARACANDRA (c. 1250).

Zur Lebensgeschichte und wissenschaftlichen Tätigkeit von Giovanni Fontana (1395?-1455?)

Der Namen von GIOVANNI FONTANA dürfte den Lesern unserer Zeitschrift schon ziemlich wohl bekannt sein. GEORGE SARTON und LYNN THORNDIKE haben ihm wiederholt ihre Aufmerksamkeit gewidmet (*Isis* VII, 1925, S. 105; XIII, 1929/30, S. 93, 103; XIV, 1930, S. 221-222; XV, 1931, S. 31-46) und man wird uns gewiss keine Uebertreibung vorwerfen, wenn wir behaupten, dass ihn dabei ein seltsames Glück begleitet hat. Denn es ist ja kein gewöhnlicher Vorfall in der Literärgeschichte, wenn im J. 1931 die Feststellung gelingt (1), ein umfangreiches, unter dem Namen eines Anderen gedrucktes Werk gehöre in Wirklichkeit einem Verfasser, von dem man noch im J. 1925 der Ansicht sein konnte, er sei ganz unbekannt geblieben (2).

Entsprach jedoch tatsächlich jene Ansicht der Wirklichkeit? Wusste man vor dem J. 1925 in der Tat nichts weiteres über FONTANA, als dass von ihm das interessante Skizzenbuch *Bellicorum instrumentorum liber* herrühre, welches sich handschriftlich in der Bayrischen Staatsbibliothek zu München erhalten hat? Zum Glück liegt die Sache nicht so schlimm. Seit dem XVIII. Jahrhundert findet man den Namen unseres Verfassers in mehreren gedruckten Werken; nur sind die betreffenden Nachrichten so weit zerstreut, dass es des Zufalls benötigte einer längeren Reihe derselben zu begegnen.

Was meine Person anbelangt, interessiert mich FONTANA seit rund 20 Jahren. Die schöne Entdeckung, welche THORNDIKE

(1) LYNN THORNDIKE, *An unidentified work by Giovanni da' Fontana : Liber de omnibus rebus naturalibus* (*Isis* XV, S. 31-46).

(2) GEORGE SARTON, *Giovanni de' Fontana* (*Isis* VII, S. 105).

zuletzt gemacht hat, gibt mir also den Anlass zur Veröffentlichung der Notizen, welche ich in diesen zwei Dezennien über die Person und die Schriften jener eigenartigen Gestalt gelegentlich angesammelt habe. Aus ihnen wird es sich m.E. ergeben, dass wir schon heute über FONTANA besser unterrichtet sind, als über mehrere seiner Zeitgenossen. Ich gebe die Hoffnung nicht auf, dass weitere Forschungen in Bibliotheken und Archiven diese Nachrichten in mehr als einer Hinsicht noch vervollständigen werden.

Das erste sichere Datum, welches wir zur Bestimmung der Lebenszeit unseres Verfassers haben, ist das Jahr 1418, zu welcher Zeit er in Padua die Artes und die Medizin studierte (s. weiter unten). Danach zu urteilen, musste er zwischen 1390 und 1400 geboren sein. Da er in allen Quellen, welche sich zweifellos auf ihn beziehen, als « Venetus » oder de « Venetiis » bezeichnet wird, so müssen wir den Versuch THORNDIKES (3) als wenig glücklich bezeichnen, ihn mit einem Dominikaner gleichen Namens und Vornamens zu identifizieren, welcher in dem bekannten Werke von QUETIF-ECHARD erwähnt wird (4), welcher aber aus Piacenza stammte und dortselbst tätig war. Denn der unsrige GIOVANNI FONTANA stammte aus Venedig, ist niemals Mönch gewesen und befasste sich mit lauter profanen Wissenschaften. Für seine Abstammung aus Venedig spricht übrigens noch der Umstand, dass er sich am 10 August 1410 in der Lagunenstadt befand, wo er den furchtbaren, auch sonst von den Geschichtschreibern erwähnten Sturmwind erlebte (5).

Mit dem Studium der Naturwissenschaften und der Technik begann sich FONTANA bereits im Jugendalter zu befassen. Die (erste ?) Frucht dieser Beschäftigung war sein *Tractatus de pisce, cane et volucre*, « quem doctissimus JOHANNES FONTANA VENETUS in adolescentia sua edidit » (s. weiter unten). Die höhere Ausbildung wurde ihm an der Universität Padua zuteil; wie bereits erwähnt, sehen wir ihn daselbst im J. 1418 « cum studuit in artibus et medicina ». Am 7. Juli desselben Jahres 1418 wurde er sogar

(3) THORNDIKE in *Isis* XIII, S. 103; XV, S. 36-37.

(4) J. QUETIF und J. ECHARD, *Scriptores Ordinis Praedicatorum*, I, Parisiis 1719, S. 772.

(5) THORNDIKE in *Isis* XV, S. 34 Anm. 13.

zum Rektor der Artisten erwählt und hielt diese Würde bis Ende Juli 1419 inne (6). Es geht daraus hervor, dass das Schuljahr 1418/9 nicht das erste gewesen sein kann, welches er an der Universität zubrachte. Daraus erklärt es sich, dass er noch die Vorlesungen von BIAGIO PELACANI DE PARMA († 1416) hören konnte (7). Ein anderer Lehrer von ihm ist der berühmte Averroist PAUL VON VENEDIG O. Aug. († 1429) gewesen (8); der hervorragende Paduaner Mathematiker und Astronom PROSDOCIMO DE' BELDOMANDI († 1428) war sein Freund (9).

Seine Studien in Padua beendigte FONTANA mit der Erlangung des Doktorgrades in der Medizin ; denn in den Ueberschriften zweier seiner Werke wird er « celeberrimus artium et medicine doctor, peritissimus dominus JOHANNES FONTANA DE VENETIIS » genannt (s. weiter unten). Er kehrte sodann in seine Vaterstadt zurück und wurde dort « medicus salariatus » der Republik; mit diesem Titel bezeichnet er sich selbst am Ende seines *Tractatus de trigono balistario*, welchen er am letzten Februar 1440 zu Udine beendigt hat (10). In Venedig scheint er auch den ganzen Rest seines ziemlich langen Lebens zugebracht zu haben, abgesehen natürlich von kürzeren oder längeren Reisen, die ihn in andere Gegenden Italiens führten. So wurde er zwischen 1420 und 1432 in das Hauptquartier der Mailänder Armee in Brescia gesandt, um dem Condottiere Grafen CARMAGNUOLA eine Botschaft des Dogen von Venedig zu überbringen (11). Ein anderes Mal hatte er die Gelegenheit einen « fornax archimisticus (= alchimisticus) » besonderer Konstruktion in Bologna kennen zu lernen (12).

(6) J. FACCIOLATI, *Fasti gymnasii Patavini*, I, Patavii 1757, S. 78. FACCIOLATI nennt ihn JOHANNES DE FONTANA.

(7) THORNDIKE in *Isis* XV, S. 33-34.

(8) THORNDIKE in *Isis* XV, S. 33. Ueber PAUL vgl. E. RENAN, *Averroès*, 3e ed., Paris 1866, S. 344-346; F. MOMIGLIANO, *Paolo Veneto*, Torino 1907; P. DUHEM, *Le système du monde*, IV, Paris 1916, S. 280-289.

(9) THORNDIKE in *Isis* XV, S. 34. Ueber PROSDOCIMO vgl. A. FAVARO, *Intorno alla vita ed alle opere di Prosdocimo de' Beldomandi* (*Bullettino Boncompagni* XII, 1879, S. 1-74, 115-251).

(10) S. weiter unten. THORNDIKE (*Isis* XIV, S. 221) bezog fälschlich das « eodem in loco » auf Udine, anstatt an das unmittelbar vorangehende « Venetum ». Der Fehler ist stillschweigend berichtigt worden in *Isis* XV, S. 37.

(11) THORNDIKE in *Isis* XV, S. 35, 42.

(12) S. J. VON ROMOCKI, *Geschichte der Explosivstoffe*, I, Berlin 1895, S. 232.

Das genaue Todesdatum unseres Gelehrten ist uns z.Z. noch nicht bekannt. Auf jeden Fall überlebte er das Jahr 1450 (13). Sein Leben erstreckte sich also ungefähr auf die Jahre 1395-1455.

Das Verzeichnis seiner Schriften umfasst über ein Dutzend von Titeln. Zehn von denselben sind uns näher bekannt geworden. Sie werden im Folgenden in einer approximativ chronologischen Ordnung aufgezählt.

1. *Tractatus de pisce, cane et volucre, quem doctissimus ... Johannes Fontana Venetus in adolescentia sua edidit.* GIROLAMO TIRABOSCHI entdeckte diesen *Tractatus* in einer Handschrift, welche sich im J. 1784 in der Libreria di S. Salvatore zu Bologna befand (14). Ueber den Inhalt desselben macht er freilich keine näheren Angaben; aus einem späteren Werke FONTANAS geht jedoch unzweideutig hervor, worum es sich darin handelt. Daselbst wird jenes Jugendwerk unseres Verfassers unter einem wenig abgeänderten Titel angeführt : *Tractatus de pisce, ave et lepore* (15). Mit dem « Fisch », dem « Hasen » (oder « Hund ») und dem « Vogel » sind hier keine lebendige Wesen gemeint, sondern drei verschiedene Arten von Raketen, die schwimmende, die rollende und die fliegende (16).

2. Dieselbe Bologneser Handschrift enthielt weiter ein Werk u.d.T. *Horologium aqueum, quod celeberrimus artium et medicine doctor, peritissimus dominus Johannes Fontana de Venetiis composuit* (17). Die zweite Hälfte dieser Ueberschrift stand freilich auf einer Rasur, so dass es nicht ausgeschlossen schien, man habe den Namen des wahren Verfassers getilgt, um das Werk fälschlich dem FONTANA beizulegen; viel wahrscheinlicher ist

(13) THORNDIKE in *Isis* XV, S. 35, Anm. 16.

(14) G. TIRABOSCHI, *Storia della letteratura italiana*, T. VI, P. II, Roma 1784, S. 462, in einer Fussnote zu S. 461. Diese Fussnote fehlt in der Ausgabe Modena 1776, S. 404. Daraus erhellt, dass die betreffende Handschrift erst in den Jahren 1776-1784 von TIRABOSCHI entdeckt worden ist. Es ist dies wahrscheinlich dieselbe Handschrift, auf welche JÄHNS anspielt, wenn er in seinem weiter unten anzuführenden Werke (S. 277) angibt, FONTANA habe « über die Brandraketen und Torpedos ein besonderes Buch geschrieben, welches die Bibliothek zu Bologna bewahren soll ».

(15) ROMOCKI *a.a.O.* S. 231. (Hier ist die Fundstelle des Zitates unrichtig als S. 97a, anstatt 37a, angegeben; vgl. ROMOCKI S. 238.)

(16) Vgl. ROMOCKI *a.a.O.* S. 237-240.

(17) TIRABOSCHI *a.a.O.* S. 461-462 (in der erwähnten Fussnote).

jedoch die andere von TIRABOSCHI zugelassene Möglichkeit, « che siasi voluto rinnovare il nome del FONTANA, che fosse omai vicino a smarrirsi». Am Ende des Werkes stand das leider unvollständige Datum : « perfectum MCCCCX... die ult. Octobr. ».

3. Dieselbe Bologneser Handschrift enthielt noch ein weiteres Werk u.d.T. *Nova compositio horologii, quod ex pulverum casu consistit, per famosissimum artium et medicine doctorem, peritissimum dominum Johannem Fontanam de Venetiis ad Ludovicum Venetum suum*, mit dem Datum: «Padue 1418 cum studuit in artibus et medicina » (18). Ob auch hier der Name des Verfassers auf einer Rasur stand, geht aus den Angaben TIRABOSCHIS nicht ganz deutlich hervor.

4. Eine unserem « JOHANNES FONTANA VENETUS » zugeschriebene Abhandlung *De speculo* fand THORNDIKE (19) im cod. Barber. lat. 350 (s. XIV-XV), fo. 61-65v. Dieselbe beginnt : « Cum inferiorum cognitio ad celestium conducat inquisitionem » und endigt : « alio modo et commodius videtis ».

Diese Nummer unseres Verzeichnisses bedarf jedoch einer noch weiteren Untersuchung. Die erwähnte Handschrift der Barberiniana (früher cod. Barber. lat. X, 168) hat nämlich bereits im J. 1902 der unvergessliche AXEL ANTON BJÖRNBO in seinen Händen gehabt und in demselben Jahre hat er auch eine kurze Notiz über dieselbe im Anhange seiner *Studien über Menelaos' Sphärik* veröffentlicht. Er berichtet hier folgendes (20) : « Diese Handschrift besteht aus mehreren Heften aus dem XIV. und dem Anfang des XV. Jahrhunderts, teils von Pergament und teils von Papier. Ueber einem anonymen, scheinbar astronomischen Text, welcher in einem Pergamentheft dieser Handschrift enthalten ist und der im XIV. Jahrhundert geschrieben ist, hat eine dem Anfang des XV. Jahrhunderts angehörige Hand folgende Worte notiert : *Libellus de speculo mukesi* (21) *magistri Johannis Fontana Venetiis* (22) *No. VIIII.* Diese Ueberschrift gehört kaum zu dem Hefte, über dem sie angebracht ist; vielmehr gehört sie zu einem weg-

(18) TIRABOSCHI *a.a.O.* S. 461 (Fussnote).

(19) THORNDIKE in *Isis* XIII, S. 93, 103.

(20) *Abhandlungen zur Geschichte der mathematischen Wissenschaften*, XIV, Leipzig 1902, S. 137.

(21) Soll « mukefi » (d.h. parabolisch) heissen. A.B.

(22) Sic ! A.B.

gefallenen Heft; denn es sind ganz deutlich mehrere Hefte aus dem Einband herausgefallen und die Numerierung der Hefte ist ganz offenbar unkorrekt ». Mit dieser Notiz stimmen nun die Originalaufzeichnungen BJÖRNBOS nicht, welche sich seit seinem frühzeitigen Tode († 1911) in meiner Verwahrung befinden. Hier sagt er folgendes : « FONTANAS vaerk kommer fol. 61r-65v. Chartac. (23). 2 kolonner. Skrift senest c. 1500 (24). Overskriften senest c. 1500. FONTANAS vaerk beg. : " Cum inferiorum cognitio ad celestium conducat inquisitionem, nam ex (25) ex hijs que apud nos sunt uelut experimentaliter apprehendimus, non ergo inferiorum orbium omnino latens erit dispositio superiorum exquirentem habitudinis connexum. Est ergo orbis terre spericitatem" etc. ». Jene Ueberschrift (*Libellus de speculo mukefi*) erwähnt hier BJÖRNBO mit keinem Worte; auch über den Inhalt des Stückes macht er keine näheren Angaben.

5. Dass sich jedoch FONTANA tatsächlich mit parabolischen Brennspiegeln beschäftigte, geht aus dem cod. Paris. lat. 9335 hervor. Von dieser Handschrift des frühen XIV. Jahrhunderts wird noch weiter unten die Rede sein. Auf fo. 134r, welches ursprünglich unbeschrieben geblieben war, stehen hier *Dicta atque notata per me Johannem Fontana physicum Venetum et probata pro declaratione speculi ardentis quedam sunt principia declaranda*(26). Das Stück ist ein Autograph FONTANAS; es nimmt nur diese eine Seite ein.

6. Noch ein anderes Werk unseres Verfassers hat THORNDIKE handschriftlich festgestellt (27). Es ist dies ein ziemlich umfangreiches Werk (223 Blätter in Grossquarto) über die Feldmesskunst (28) und führt den Titel *De trigono balistario*. Die betref-

(23) Also kein « Pergamentheft ». A.B.

(24) Also nicht « im XIV. Jahrhundert geschrieben ». A.B.

(25) Sic ! Soll « ea » heissen. A.B.

(26) So hat BJÖRNBO gelesen (*Bibliotheca Mathematica*, 3-te Folge, III, 1902, S. 66, 74, wozu die Berichtigung in den *Abhandlungen zur Geschichte der mathematischen Wissenschaften*, *a.a.O.*, zu vergleichen ist). Da aber BJÖRNBO im Jahre 1901-2, als er die Handschrift in München benutzte, erst ein angehender Paläograph gewesen ist, so ist es möglich, dass er nicht alles richtig zu entziffern vermochte.

(27) THORNDIKE in *Isis* XIV, S. 221-222.

(28) Wenigstens ein Teil des Werkes muss auch astronomischen Inhalts sein, wie dies aus der folgenden Anführung desselben hervorgeht, welche sich in dem *Liber de omnibus rebus naturalibus* findet : « Ego similiter, cum... tractatum...

fende Handschrift, cod. Oxon. Canon. Misc. 47, scheint die Originalhandschrift des Verfassers zu sein und enthält mehrere geometrische Zeichnungen. Das Werk ist einem Venetianer DOMENICO BRAGADINO gewidmet; die Anfangsworte der Vorrede, sowie den Anfang und den Schluss des Werkes selbst wolle man in der *Isis* XIV S. 222 nachsehen. Das Kolophon lautet : « Editus et expletus est tractatus iste *De trigono balistario* anno gratie M°CCCC°XL° in Utino die ultimo Februarii per JOHANNEM FONTANAM VENETUM, physicum medicum eodem in loco salariatum » usw.

7. Viel besser, als über die bisher aufgezählten Schriften FONTANAS, sind wir über ein weiteres, recht interessantes Werk von ihm unterrichtet, welches sich handschriftlich in der Bayrischen Staatsbibliothek zu München erhalten hat. Die betreffende Handschrift (cod. iconogr. 242), ein reich durch Federskizzen illustriertes Brouillon des Verfassers, besteht aus 70 Pergamentblättern im mittleren Folio-Format und führt keinen ursprünglichen Titel; der heutige rührt von einem der früheren Besitzer her und lautet : *Bellicorum instrumentorum liber cum figuris et* (!) *fictitiis literis conscriptus*. Die « fictitiae literae » sind ein Geheimalphabet, dessen sich FONTANA bediente, um die ihm besonders wichtig erscheinenden Dinge niederzuschreiben. Als kriegswissenschaftlich ist das Buch so ziemlich mit Recht bezeichnet; denn obgleich es auch Vieles enthält, was mit dem Krieg in einem nur sehr mittelbaren Zusammenhange steht, bleibt doch immer kriegerische Verwendung der leitende Gedanke des Verfassers.

Uebrigens wird FONTANA weder am Anfang noch am Schluss dieses seinen Skizzenbuches als Verfasser genannt, so dass die Handschrift eigentlich nicht nur titellos, sondern auch anonym ist. Dass aber das Werk tatsächlich dem Venediger Arzte gehöre, ersieht man nicht nur daraus, dass sich der Verfasser auf seinen früher geschriebenen *Tractatus de pisce, ave et lepore* beruft, sondern auch und vor allen Dingen daraus, dass man auf S. 10a und S. 22b den folgenden Stellen begegnet : « sicut ego JOHANNES DE (!) FONTANA pinxi ... » bezw. « Ego JOHANNES FONTANA novos

componerem... *De trigono balistario* instrumento novissimo, in similes errores incidi, sequens ALFONSI canones et aliorum vestigia » (THORNDIKE in *Isis* XV, S. 36, Anm. 18).

adinveni fontes ... » usw. Ein Datum findet sich in der Handschrift nicht; MAX JÄHNS (29) und S. J. VON ROMOCKI (30) verlegen sie auf « etwa 1420 ». Ich würde sie vielmehr auf die zweite Lebenshälfte des Verfassers versetzen, zumal sich derselbe darin auf mehrere seiner früheren (schriftstellerischen und praktischen) Arbeiten beruft.

Auf den Inhalt des « Skizzenbuches » näher einzugehen dürfte hier nicht nötig sein; es genügt ein Verweis auf die Berichte von JÄHNS und ROMOCKI (31).

8. *Secretum de thesauro experimentorum ymaginationis hominum.* Im Jahre 1897 hat die Pariser Nationalbibliothek von einem Buchhändler in Venedig eine Pergamenthandschrift des XV. Jahrhunderts erworben, welche fast ganz in einer Geheimschrift geschrieben ist; nur in dem Titel, in dem Inhaltsverzeichnis und in dem nichtssagenden Kolophon (« Et sic est finis huius operis cum benedictione Dei. Amen ») wurde die gewöhnliche lateinische Schrift angewandt. Der Titel lautet : « Incipit Secretum de thesauro experimentorum ymaginationis hominum, quod ////// taliter opinatus est et sub compendio conscripsit ». Nach « quod » stand offenbar ursprünglich der Name des Verfassers; nachher wurde jedoch derselbe ausradiert. Daraus erklärt es sich, dass man zunächst nicht wusste, von wem das Werk sei. Als anonym behandelte es deshalb HENRI OMONT in seiner Abhandlung *Un traité de physique et d'alchimie du XV^e siècle en écriture cryptographique* (32). Erst in dem nächstfolgenden Jahre gelang es ihm (auf eine nicht näher angegebene Weise) den vermutlichen Verfasser zu entdecken; in dem kurzen Inventar der *Nouvelles acquisitions du Département des Manuscrits de la Bibliothèque Nationale pendant les années 1896-1897* (33) setzte er deswegen nach dem Rasurzeichen in Klammern « JOHANNES FONTANA ? » hinzu (34). Dass diese Vermutung tatsächlich zutrifft, geht daraus hervor, dass die in der Handschrift angewandte Geheimschrift

(29) M. JÄHNS, *Geschichte der Kriegswissenschaften, vornehmlich in Deutschland*, I, München 1889, S. 276.
(30) ROMOCKI *a.a.O.* S. 232.
(31) JÄHNS *a.a.O.* S. 276-277; ROMOCKI *a.a.O.* S. 231-240.
(32) *Bibliothèque de l'École des Chartes* LVIII, 1897, S. 253-258.
(33) *Bibliothèque de l'École des Chartes* LIX, 1898, S. 81-135.
(34) *A.a.O.* S. 92.

im Grossen und Ganzen mit der von cod. Monac. iconogr. 242 übereinstimmt. «Es bestehen zwar einige geringfügige Differenzen, doch kann die Geheimschrift des cod. iconogr. 242 mit Hilfe des in der *Bibliothèque de l'Ecole des Chartes* LVIII S. 254 veröffentlichten Schlüssels ohne viel Schwierigkeiten entziffert werden» (35).

Die betreffende Handschrift (No. 635 du fonds latin des nouvelles acquisitions) bildet einen verhältnismässig dicken Band (140 Pergamentblätter) im winzig kleinen Format (85 × 62 mm.). Sie ist mit mehreren sorgfältigen Zeichnungen ausgeschmückt. Das Inhaltsverzeichnis druckt OMONT ab (36); wegen einer spezifischen («geheimen») Terminologie ist jedoch dasselbe recht schwierig zu verstehen.

9. *Liber de omnibus rebus naturalibus*. Dieses Hauptwerk unseres Verfassers hat ein seltsames Schicksal erlebt, worüber LYNN THORNDIKE vor kurzem in dieser Zeitschrift einen ausführlichen Bericht veröffentlicht hat (37). Vollständigkeitshalber müssen wir jedoch auch hier seine Ergebnisse in aller Kürze wiederholen.

Der *Liber de omnibus rebus naturalibus quae continentur in mundo, videlicet coelestibus et terrestribus necnon mathematicis, et de angelis motoribusquae* (!) *coelorum*, wie der vollständige Titel der gedruckten Ausgabe lautet, erschien im J. 1544 bei OCTAVIANUS SCOTUS zu Venedig unter dem Namen eines gewissen POMPILIUS AZALUS PLACENTINUS, welcher sein angebliches Werk dem Kaiser KARL V gewidmet hat. THORNDIKE hat jedoch den Beweis erbracht, dass der eigentliche Verfasser in der ersten Hälfte des XV. Jahrhunderts gelebt haben muss und mit unserem GIOVANNI FONTANA identisch ist. Ausschlaggebend für diesen Beweis war der Umstand, dass der Verfasser zu Padua am Anfang des XV. Jahrhunderts studierte und dass er sich auf sein eigenes

(35) Aus einem Briefe von Prof. GEORG LEIDINGER an den Verfasser. Hr. ERNST BORCHERT (München) schrieb mir ergänzend: «Nur in einer, vielleicht auch zwei Buchstabenverbindungen scheint eine Weiterbildung, aber vollkommen innerhalb des Systems stattgefunden zu haben. Im wesentlichen ist das System des cod. Parisinus beibehalten». Den beiden Herren spreche ich auch an dieser Stelle meinen besten Dank aus.

(36) *A.a.O.* S. 256-258.

(37) LYNN THORNDIKE, *An unidentified work by Giovanni da' Fontana* (*Isis* XV, 1931, S. 31-46).

Werk *De trigono balistario* beruft; vgl. die Nummer 6 unseres Verzeichnisses.

Der *Liber* ist ein recht umfangreiches Werk; in der genannten Druckausgabe nimmt er (die Vorrede von AZALUS miteingerechnet) 142 Folioblätter ein. Er zerfällt in fünf Teile und sein Inhalt bezieht sich auf die Astronomie, auf die Meteorologie, auf die Geographie, auf die Mineralogie, auf die Botanik und auf die Zoologie; auch philosophische, theologische und astrologische Fragen werden behandelt (38). Im Grossen und Ganzen ist das Werk eine « combination of science and superstition, of credulity and correct information » (39).

Das Werk gehört der Spätzeit seines Verfassers an; es wurde nach dem Traktate *De trigono balistario* (1440) geschrieben und erwähnt das Jahr 1450 als bereits verflossen (40).

10. Ein medizinisches (?) Rezept « magistri JOHANNIS FONT. de Veneciis » befand sich auf dem Vorsetzblatt der Handschrift Nr. 101 (147) der ehemaligen Bibliothek des Fürsten B. BONCOMPAGNI in Rom (41). Da diese Handschrift aus dem Anfang des XVI. Jahrhunderts stammte, ist die Zuweisung dieses Rezeptes an unseren JOHANNES FONTANA nicht ganz sicher, wenngleich sehr wahrscheinlich.

Mit den bisher aufgezählten Werken FONTANAS ist jedoch seine schriftstellerische Tätigkeit noch nicht erschöpft. In

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(38) Vgl. die Inhaltsangabe bei THORNDIKE *a.a.O.* S. 33-34 sowie seine Auszüge aus dem geographischen Teil, S. 37-42. Ueber die astronomischen, astrologischen und magischen Kenntnisse, deren FONTANA im *Liber* Zeugnis ablegt, vgl. THORNDIKE daselbst S. 42-46. Ich füge noch hinzu, dass der anonyme Optiker, von dem FONTANA behauptet (lib. III c. 14), er habe bewiesen, dass der Radius des Regenbogens 44° beträgt (THORNDIKE, S. 43), entweder mit ROGER BACON (vgl. *Opus Maius* VI, c. 4 und c. 6) identisch sein wird, oder aber mit dem (gleichfalls anonymen) Verfasser eines noch nicht wiedergefundenen Traktates *De iride et de halo demonstrative* mit dem Anfang : « Cum expertum sit per astrolabium », welcher sich im XIV. Jahrhundert in der Handschrift « V 1 » der Bibliothek der Sorbonne zu Paris befand; vgl. A. BIRKENMAJER, *Studja nad Witelonem*, I, Kraków 1921, S. 117 Anm. 2. Bei den beiden findet sich freilich die Zahl 42°, und nicht 44°.

(39) Vgl. THORNDIKE *a.a.O.* S. 46, welcher unterstreicht, dass sich ein mehr eingehendes Studium des *Liber* wahrscheinlich noch löhnen würde.

(40) THORNDIKE *a.a.O.* S. 35, Anm. 16.

(41) E. NARDUCCI, *Catalogo di manoscritti ora posseduti da D. Baldassarre Boncompagni*, Seconda edizione, Roma 1892, S. 64. In dem Versteigerungskataloge der Bibliothek (1898) trug die fragliche Handschrift die Nummer 78.

seinem «Skizzenbuche» sowie in seinem Hauptwerke *De omnibus rebus naturalibus* erwähnt er nämlich Titel von mehreren seiner Schriften, welche man bisher noch nicht aufgefunden hat. Wir stellen die betreffenden Zitate zusammen, in der Hoffnung, damit zur Identifizierung dieser Schriften, die sich möglicherweise anonym in den Handschriften finden werden, beizutragen.

11. *Libellus de laberintis*. Erwähnt in dem «Skizzenbuche» S. 10a, bei der Gelegenheit einer Zeichnung, welche den Grundriss eines viereckigen Irrgartens veranschaulicht : « De laberintis non datur ordo; sed quamvis laberintus iste sit quadratus, potest in omnem modum angulorum fieri, sicut ego JOHANNES DE FONTANA pinxi in *Libello de laberintis* secundum queque genera figurarum » usw. (42).

12. Eine Abhandlung über die Brunnen (Springbrunnen?). Dass FONTANA eine solche Abhandlung geschrieben hat, geht m.E. aus der folgenden Stelle seines «Skizzenbuches» (S. 22b) hervor : « Ego JOHANNES FONTANA novos adinveni fontes, partim ex antiquorum fundamentis collectos, partim ex proprio ingenio; et hunc inter ceteros laudo, quem longo et prolixo sermone alias cogitavi » usw. (43).

13. Einen, wie es scheint, von der letztgenannten Abhandlung zu unterscheidenden *Libellus de aqueductibus* erwähnt Fontana in seinem «Skizzenbuche» S. 48 (44).

14. *Canones artis pictorie, ad Jacobum Bellinum Venetum.* Angeführt in dem *Liber de omnibus rebus naturalibus* lib. III c. 14 (ed. 1544 fo. 74v.) : « Ab hac naturali experientia ars pictoria optimos canones accepit, ut in libello ad JACOBUM BELLINUM VENETUM pictorem insignem certe descripsi » (45).

15. *De sphaera solida* (Ueber den Himmelsglobus). Angeführt in dem *Liber de omnibus rebus naturalibus*, ed. 1544 fo. 36r. : « Ego similiter, cum quendam tractatum *De spaera solida* componerem » usw. (46).

Das von QUETIF-ECHARD erwähnte (aber nicht gesehene) *Opus in quo multa moralia* muss dagegen aus der Bibliographie unseres

(42) ROMOCKI *a.a.O.* S. 231.
(43) ROMOCKI *a.a.O.* S. 231-232.
(44) ROMOCKI *a.a.O.* S. 231.
(45) THORNDIKE in *Isis* XV, S. 36, Anm. 17.
(46) THORNDIKE, *ibidem*, S. 36, Anm. 18.

Verfassers endgiltig gestrichen werden. Denn auch in diesem Falle, wenn die betreffende Nachricht nicht auf irgendwelcher Konfusion beruht (was keineswegs ausgeschlossen zu sein scheint), bezieht sich dieselbe auf den angeblichen Dominikaner JOHANNES FONTANA PLACENTINUS, welcher mit unserem Venediger Arzte nichts weiteres als die Gleichheit des Namens gemein haben kann. Auf jeden Fall darf das verlorengegangene *Opus* nicht mit dem *Liber de omnibus rebus naturalibus* gleichgesetzt werden(47).

Ueberblicken wir das ganze Lebenswerk von GIOVANNI FONTANA dem Venediger, soweit uns dasselbe aus den oben zusammengestellten, noch recht fragmentarischen Nachrichten entgegentritt, so müssen wir feststellen, dass er eine ganz eigenartige Persönlichkeit gewesen sei. Das Interesse für das Technische, für die praktischen Anwendungen der Mathematik und Physik, für die « scientia experimentalis » im Sinne ROGER BACONS, steht bei ihm im Vordergrunde. Die Feldmesskunst, geometrische Spiele (« Irrgarten »), die Malerkunst, das Verfertigen von Himmelsgloben, von Wasser- und Sanduhren, von Brennspiegeln, von « magischen Laternen », von Kunstbrunnen, von Wasserhebevorrichtungen, von Wasserleitungen, von Vexiergefässen, von Pulverraketen, von Geschützen (« Bombarden »), von alchemistischen Oefen, u.s.w. — sind Gegenstände, mit denen er sich unermüdlich immer wieder befasst. Aus dem Bereiche der « Experimentierkunst » fällt lediglich der *Liber de omnibus rebus naturalibus* heraus, in welchem er sich mehr auf die Seite der Naturbeschreibung stellt.

Die interessante Frage, inwieweit FONTANA bei seinen Versuchen « originell » gewesen sei, hat bereits ROMOCKI aufgeworfen und auch, wie es mir scheint, sehr treffend mit dem folgenden Urteil beantwortet (48) : « Nicht an allem, was er seinem "proprium ingenium" zu verdanken angiebt, ist mehr, als eine unwesentliche Abänderung, sein geistiges Eigentum; er lehnt sich ersichtlich stets an ältere Quellen an, doch ist er auch nirgends ausschliesslich Kopist : er versteht, was er in Büchern oder in der Praxis gesehen hat, und er strebt danach, es zu vervollkommnen. »

Es wird freilich die Sache der weiteren Forschung sein, dieses

(47) Entgegen THORNDIKES, übrigens schwankendem Urteil, *Isis* XV, S. 37.
(48) ROMOCKI *a.a.O.* S. 234.

allgemeine Urteil im einzelnen zu begründen, besonders was die noch nicht genügend studierten Werke unseres Verfassers anbelangt. Dass FONTANA viel gelesen hat, kann nicht in Abrede gestellt werden. Manche der literarischen Quellen, auf welche sich sein *Liber de omnibus rebus naturalibus* stützt, hat THORNDIKE blossgelegt; für gewisse seiner kleineren Abhandlungen liegt es nahe ganz bestimmte Werke seiner Vorgänger als jene Quellen zu vermuten (49). Dazu kommt jedoch noch der glückliche Umstand hinzu, dass wir nicht allein durch die schriftstellerische Tätigkeit FONTANAS darüber (wenigstens zum Teil) unterrichtet sind, was für Autoren er in seiner Bibliothek besass.

Aus jener Bibliothek, die zweifellos eine recht umfangreiche gewesen sein musste, sind uns nämlich bis auf den heutigen Tag wenigstens zwei Handschriften erhalten. Beide sind bereits von BJÖRNBO nachgewiesen worden. Die eine von ihnen ist keine andere, als der äusserst wertvolle cod. Paris. 9335 (früher Suppl. 49). Derselbe wurde von BJÖRNBO im J. 1902 ausführlich beschrieben (50), wozu dann derselbe Gelehrte noch zwei Nachträge veröffentlicht hat(51). Die (Pergament-) Handschrift ist nach BJÖRNBO in Italien im XIV. Jahrhundert geschrieben worden. Sie enthält nicht weniger als 26 verschiedene Texte, welche sämtlich von GERHARD VON CREMONA oder seinen Schülern aus dem Arabischen übersetzt worden sind. Inhaltlich bilden diese 26 Texte vier besondere Gruppen und zwar : 1. die sogn. mittleren Bücher ; 2. vier optische Schriften ; 3. algebraische und geometrische Werke arabischen Ursprungs; 4. verschiedenes (AL-KINDÎ *De quinque essentiis* und *De gradibus compositarum medicinarum*, ein anonymes *Capitulum cognitionis mansionis Lunae*, THĀBIT IBN QURRA *De motu octavae sphaerae*, AL-FĀRĀBĪ *De scientiis*, 'ARĪB IBN SA'D *Liber anoe*). Nach dem Schluss des dritten Teiles stand ursprünglich ein leeres Blatt (fo. 134); die Vorderseite des-

(49) So z.B. PHILO für die Vexiergefässe (ROMOCKI S. 233-234), ALHAZEN für die Brennspiegel usw.

(50) A. A. BJÖRNBO, *Ueber zwei mathematische Handschriften aus dem vierzehnten Jahrhundert* (*Bibliotheca Mathematica*, 3-te Folge, III, 1902, S. 63-75).

(51) A.A. BJÖRNBO, *Studien über Menelaos' Sphärik* (*Abhandlungen zur Geschichte der mathematischen Wissenschaften*, XIV, Leipzig 1902, S. III-154), S. 137-138; ALKINDI, TIDEUS und PSEUDO-EUKLID, *Drei optische Werke*, herausgegeben und erklärt von A. A. BJÖRNBO und S. VOGL (*Abhandlungen* usw., XXVI : 3, Leipzig 1912), S. 138.

selben wurde nachträglich durch FONTANA für die Unterbringung seiner *Dicta atque notata pro declaratione speculi ardentis* benutzt (vgl. oben die Nr. 5 des Verzeichnisses seiner Werke). Auch sonst in der Handschrift finden sich Spuren einer Benutzung durch FONTANA (52). Es liegt die Vermutung nahe, dass er im besonderen den *Liber mensurationis* von ABŪ BAKR (samt den Anhängen, fo. 116v.-126v. der Handschrift) für seinen Traktat *De trigono balistario* verwertet habe, und vielleicht auch die vier (freilich den Brennspiegel nicht behandelnden) optischen Werke für seine Abhandlung *De speculo mukefi*.

Die zweite von BJÖRNBO nachgewiesene Handschrift aus der Bibliothek FONTANAS ist der cod. Digby 47 der Bodleiana in Oxford. Auf dem letzten Blatt trägt derselbe die Unterschrift: «Iste liber est JOHANNIS FONTANA physici Veneti» (53). Die Handschrift ist astrologischen Inhalts; für die Vermutungen, inwieweit dieselbe durch FONTANA in seinen eigenen Werken benutzt sein konnte, geben die gedruckten Inhaltsangaben (54) wenig Raum.

Weitere Nachforschungen werden zweifellos noch mehr Licht über das Leben, die Werke und die Bibliothek FONTANAS verbreiten. Mögen die vorstehenden Zeilen dazu beitragen, durch Zusammenstellung des weit zerstreuten Materials die nötige Grundlage für die künftige, eingehende Monographie des nahezu vergessenen Mannes zu schaffen.

NACHTRÄGE UND BERICHTIGUNGEN.

Nachdem das Manuskript der vorliegenden Abhandlung an die Redaktion der *Isis* abgesandt wurde, wurde mir durch eine Reise nach Paris die Gelegenheit geboten weiteres Material für die Lebensgeschichte und für die Bibliographie der Werke FONTANAS zu sammeln. Ich veröffentliche es in aller Kürze.

Was zuerst die Lebensumstände unseres Arztes anbelangt, so

(52) BJÖRNBO *a.a.O.* S. 66. BJÖRNBO hat ursprünglich «PONTANA» gelesen; er verbesserte dies jedoch noch in demselben Jahre 1902 (*Abhandlungen* usw. XIV, S. 137).

(53) A. A. BJÖRNBO, *Hermannus Dalmata als Uebersetzer astronomischer Arbeiten* (*Bibliotheca Mathematica*, 3-te Folge, IV, 1903, S. 130-133), S. 131.

(54) G. D. MACRAY, *Catalogus codicum manuscriptorum Bibliothecae Bodleianae*, Pars IX, Oxonii 1883, Sp. 42; bezw. *Catalogi librorum manuscriptorum Angliae et Hiberniae in unum collecti*, T. I pars I, Oxonii 1697, S. 79.

findet sich in dem *Liber de omnibus rebus naturalibus* lib. V cap. 23 ein weiterer Beweis für die Tatsache, dass sich FONTANA eine Zeit lang in Udine aufgehalten hat (55). Auch die Gegend von Ravenna hat er besucht (56). Interessanter ist die Stelle, welche bezeugt, dass er die Seereise nach der (damals zu Venedig gehörigen) Insel Kreta gemacht hat (57). Auch scheint es, dass er einmal die Alpen nordwärts passiert hat (58).

Ueberhaupt ist der *Liber de omnibus rebus naturalibus* noch reichlicher an Berichten über persönliche Erlebnisse des Verfassers, als man dies auf Grund der Abhandlung von THORNDIKE schliessen könnte. Die meisten dieser Stellen sind auch sonst von Interesse; es ist mir jedoch nicht möglich dieselben hier sämtlich anzuführen (59).

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(55) Ed. 1544 fo. 127*r* : « Sed ipse in Utino vidi quandam foeminam locantem ova gallinarum electa sparsim super stratum fimi equi, deinde superponere fimum similem et desuper alium ordinem ovorum et iterum cum fimo cooperire, et sic gradatim, et ad dies paucos pullos habere secundum numerum ovorum ».

(56) Lib. V cap. 16 (ed. 1544 fo. 117*r*). « Item apud Fontes Sancte Marie de Bagne in valle quadam ultra Ravenam vidi pluries egredi ab aqua et saxo, ex quo scaturiebant (!), flammulas ignis rubicundas, a quibus candelam accendi eque facile, ut a flamma cerei ».

(57) Lib. V cap. 26 (ed. 1544 fo. 130*v*) : « Tacere hoc in loco non possum, quod bis vidi et pluries audivi, immanitatem scilicet et impiam iniquitatem quarundam mulierum Cretensium, diabolica inspiratione noviter natos... ad evitandam infamiam nocte secrete eiicere, ... ut a porcis famelicis sub divo [Druck : clivo] vagantibus ante diem devorentur ... Et quod deterius est, cum hoc notissimum sit, remanet etiam virorum negligentia, qui pro evitando tanto periculo innocentium ... hospitale pietatis non edificant. Quod persaepe persuasi et alias persuasum fuit, et hactenus lapis primus pro fundamento iactus non est ».

(58) Lib. III cap. 20 (ed. 1544 fo. 82*r*) : « Accidit hoc frequenter in maribus et fluminibus septentrionalibus tempore gelido, quibus tanta insit aliquando durities atque grossities (ut saepe vidimus), quod non solum pedites, sed etiam desuper equites armati pergunt et currus gravissimi veluti super solidam terram ducuntur ».

(59) Einige wenige Proben mögen hier dennoch folgen. Lib. V cap. 17 (ed. 1544 fo. 118*v*) : « Et ipse memor sum, nec mentior, eo die, quo Venetiis in carcer ductus fuit comes FRANCISCUS dictus CARMIGNOLA, mane per horam fere continuam tot super urbem [Druck : orbem] nostram in aere vidisse locustas, ut neminem a me distantem per passus X cernere valerem ». Die Verhaftung erfolgte im J. 1432. Lib. V cap. 18 (ed. 1544 fo. 119*r*) : « Ego certe verus testis sum et vivunt etiam plures ex medicis qui viderunt, ⟨quod⟩ quaedam nobilis domina DATHA BOLDEMERIO Venetiis inhabitans prope traiectum Sancti Thome ex commestione pomi, cum cenasset, eadem nocte ventrem dolore intensissimo cepit tumere; crescebat tumor et dolor atque duricies in tantum, quod in horis vigintiquatuor et extincta et petrificata fuit taliter, quod post eius mortem aliquo ingenio nec iuncture flecti, nec gladio venter incidi poterat ». Lib. V cap. 28 (ed. 1544 fo. 133*r*) :

Dass FONTANA im J. 1454 wahrscheinlich noch am Leben war, werden wir weiter unten sehen.

Zu dem Verzeichnisse seiner Schriften vermag ich keinen weiteren Titel hinzuzufügen, dafür bin ich in der Lage die oben zusammengestellten Nachrichten über einige derselben ein wenig zu vervollständigen.

Nr. 1-3. Die von TIRABOSCHI entdeckte Handschrift befindet sich heutzutage in der Universitätsbibliothek Bologna, cod. lat. 1426 (2705), und ist vor zwanzig Jahren von L. FRATI beschrieben worden (60). An erster Stelle (fo. 1-50) enthält sie die *Nova compositio horalegi* (!) *quod ex pulverum casu consistit* (= Nr. 3), dann folgt (fo. 53-75) das *Horalegium* (!) *aqueum* (= Nr. 2) und endlich (fo. 85-105) der *Tractatus de pisce, cane et volucre* (= Nr. 1). Zwischen der Beschreibung FRATIS und den Angaben TIRABOSCHIS bestehen gewisse kleine Differenzen, auf die ich jedoch nicht näher eingehen kann; auffallend finde ich nur, dass FONTANA (nach FRATI) sowohl in der Ueberschrift, als auch in dem Kolophon von *Horologium aqueum* « JACOBUS » und nicht « JOHANNES » genannt wird. Vielleicht steht der Name auf der von TIRABOSCHI festgestellten Rasur (welche aber FRATI mit keinem Worte erwähnt). Die Handschrift ist mit Federzeichnungen ausgeschmückt.

Nr. 5. Die Zahl der im cod. Paris. lat. 9335 enthaltenen Werke beträgt in Wirklichkeit 28, und nicht 26. Unter der Nr. 14 hat nämlich BJÖRNBO drei verschiedene Werke zusammengefasst, die Abhandlung von TIDEUS (fo. 84*ra*-85*va*), das Bruchstück von APOLLONIUS (fo. 85*va*-86*ra*), sowie die anonyme Abhandlung *De speculo comburenti* (inc. « De sublimiori quod geometre adinvenerunt », fo. 86*ra*-88*va*), welche von dem arabischen Optiker

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« FRANCISCHUS DE KARARIA senior, dominus Patavinae civitatis, tantam de prole curam habuit, ut ne ab ancilla concubina sua, muliere speciosissima, falleretur, serabulam ferream ingenio strui fecit, ut [Druck : et] ea semper cum vellet et mingere ⟨et⟩ egere posset, coyre nequaquam cum alio viro ab ipso domino suo. Et hoc instrumentum ipse vidisse fateor ». Wir haben hier ein neues, m.E. entscheidendes Zeugnis für die Glaubwürdigkeit der Version, nach welcher der berühmte Keuschheitsgürtel von dem Tyrannen FRANCESCO DA CARRARA IL VECCHIO (Herrscher von Padua bis 1388, gest. 1393) zuerst erfunden worden sei; vgl. E. FUCHS, *Illustrierte Sittengeschichte*, I, München 1909, S. 337.

(60) L. FRATI, *Indice dei codici latini conservati nella R. Biblioteca Universitaria di Bologna*, Firenze 1909, S. 525 (= *Studi Italiani di Filologia Classica*, XVII, 1909, S. 93).

ALHAZEN herrührt (61). Die *Dicta atque notata* FONTANAS beziehen sich eben auf das letztgenannte Werk. Am Rande von fo. 88*rb* (also gegen das Ende des Werkes) schrieb er nämlich folgende Worte nieder : « In hoc libro in carta 134 ego JOHANNES FONTANA posui 3 formas extractas ex eadem pyramide ex sectione mukefi secundum artem predictam ». Diese Randbemerkung bezieht sich auf die Stelle des Textes, welche lautet (62) : « Hec est summa sermonis in opere speculi concavi, quod est secundum figuram composicionis mukefi. Qualiter autem faciam speculum concavum comburentem... Et imaginabimur longitudinem quesitam sicut .dz. et protrahemus in lamina sectionem ex sectione mukefi... », und speziell auf das Wort « sectionem ». Das auf fo. 134*r* stehende Stück ist demnach eine von FONTANA herrührende Ergänzung zu dem Werke ALHAZENS. Es beginnt : « Dicta atque notata per me JOHANNEM FONTANAM physicum Venetum et probata. Pro declaratione speculi ardentis quedam sunt principia declaranda. Pyramis orthogonia rotunda est... ». Auf die Definitionen folgt die Anweisung zur Erzeugung der gesuchten Parabel : « Formas pro speculo fiendo sic accipies. Invenias in pyramide rectangula... Et illa sectio pyramidis... est sectio mukefi vel parabola vel ortogonia, quod idem est. Et hec sunt que declarare voluimus. Reliqua habentur in tractatu ». Die Notiz ist von drei Figuren begleitet.

Nr. 8. Das Inhaltsverzeichnis des *Secretum de thesauro experimentorum ymaginationis hominum* ist von H. OMONT nicht ganz genau abgedruckt worden. Anstatt « distinctio » (« distinctionis » usw.) ist überall « diccio » (« diccionis » usw.) zu lesen; S. 256 Z. 5 lese man « universali » und nicht « vulgari », Z. 13 « istius » und nicht « illius ». S. 257 Z. 24 steht nach « literis » noch ein Wort, welches ich jedoch nicht entziffern konnte.

Die Handschrift (Nal. 635) gehört zu der sogn. « réserve » und kann deswegen nur an Ort und Stelle in Paris studiert werden. Auf der anderen Seite würde die vollständige photographische Aufnahme des dicken Bandes unverhältnismässig viel kosten. Derjenige, der das Werk etwa edieren wollte (63), wird also gut

(61) Vgl. J. L. HEIBERG und E. WIEDEMANN, *Ibn al Haitams Schrift über parabolische Hohlspiegel* (*Bibliotheca Mathematica*, 3-te Folge, X, 1909-1910, S. 201-237).

(62) Ed. HEIBERG-WIEDEMANN, S. 229, Z. 24 ff.

(63) THORNDIKE möchte ja die Werke FONTANAS in das von ihm geplante *Corpus of Medieval Scientific Literature* einverleiben; vgl. *Isis* XIV, 1930, S. 377.

tun, das Geheimalphabet FONTANAS schon vor der Reise nach Paris gründlich einzuüben (am besten auf Grund von Aufnahmen aus dem cod. Monac. icon. 242). Ich mache ihn darauf aufmerksam, dass die Gestalt der einzelnen Buchstaben von Fall zu Fall ein wenig variiert (für den Buchstaben *m* habe ich z.B. drei verschiedene Formen notiert).

Nr. 9. Der Druck von 1544 zählt 143 und nicht 142 Blätter. Das Blatt mit der Vorrede von AZALUS ist unfoliert. Darnach folgen richtig die Bll. 1-38, die Numerierung der folgenden Blätter ist dagegen voll von Fehlern. Bis fo. 58 fehlen die Bll. 39, 44, 47, 53, 57, wofür die Bll. 41, 46 und 55 doppelt vorkommen. Auf fo. 58 folgen 16 Blätter bezeichnet mit den Nummern 60, 59, 60, 61, 61 (!), 63, 66, 65, 65 (!), 66, 73, 78, 69, 70, 71, 78 (!). Jetzt beginnt die fortlaufende Numerierung mit Bl. 72 (73 usw.), doch fehlen bis zum Schluss des Bandes (fo. 142) die Bll. 78, 98, 123, 130, wofür die Bll. 96 und 131 doppelt vorkommen. Ganz am Ende steht noch ein unfoliiertes Blatt (mit dem Signet des Druckers).

Die beiden ersten Bücher (fo. 1*r*-73*v*) sind astronomisch-astrologischen Inhalts, das dritte (fo. 73*v*-89*v*) enthält die Meteorologie (« De elementis et quae in eis generantur »), das vierte (fo. 89*v*-104*v*) die Geographie (« De locis habitabilibus »), während das letzte (fo. 105*r*-142*v*) « De mirabilibus rebus naturalibus » betitelt ist. Die — oben bereits angeführte — Bewertung des Werkes durch THORNDIKE wäre dahin zu ergänzen, dass für die superstiziösen Elemente lediglich die Quellen verantwortlich sind, aus denen FONTANA schöpfte. Dass er Fabeln wiederholt, welche er in seinen « auctores » gefunden hat, erklärt sich aus dem damaligen Stande des Wissens; man könnte dagegen eine ganze Reihe von Stellen anführen, aus denen hervorgeht, dass er selbst eine nüchterne, kritisch angelegte Natur gewesen sei.

An dem ersten Buche arbeitete der Verfasser, wie es scheint, im J. 1454. Denn wir lesen im 6-ten Kapitel (fo. 6*v*) : « Nec illis credendum est, qui ... fatentur, quod mundus factus sit ante adventum Salvatoris per annos quinque milia et ducentos vel circa; et hodie sumus in annum (!) gratiae milesimo quingentesimo XXXVI post Christi nativitatem, quare a principio mundi usque ad praesens transacti sunt anni sex milia setticenti (!) et quinquaginta et quatuor vel circa; et forsitan erit mundi finis

usque ad annos duodecim vel saltem apparitio ANTICHRISTI ». Die Jahreszahl 1536 des zweiten Absatzes stammt offenbar von dem Ueberarbeiter (AZALUS) her; berücksichtigt man dagegen nur den ersten und den dritten Absatz, so kommt man auf das Jahr 6654—5200=1454. Der vierte Absatz bestätigt die Richtigkeit der Zahl 6654, denn 6654 + 12 = 6666, d.h. die erweiterte « apokalyptische » Zahl.

Auf die zahlreichen Zitate des *Liber de omnibus rebus naturalibus* kann ich hier nicht näher eingehen; zwei von denselben verdienen jedoch besondere Aufmerksamkeit. Lib. I cap. 22 (fo. 22*r*): « Hac experiencia cognita ab architectis artificibus inventus et cognitus fuit fons ex tribus cannis iucunditate et iucositate (!) participans. Atque multa ingeniosa, quae CHYRON (!) *De ductibus aquarum* commemorat, eodem (!) praeternaturali aeris pressione pulchrum effectum habent atque mirandum ». Lib. V cap. 4 (fo. 107*r*): « Et quoniam talis chylindrus est in propositione (!) sesquialtera ad ipsam spheram, per octavam ARCHIMENIDIS *De cononydalibus* (!) *et spoydalibus* (!), demta tertia parte remanebit quantitas corpulentiae sphaere ». Um die Mitte des XV. Jahrhunderts verschaffte sich also FONTANA Zugang zu der *Pneumatik* HERONS und zu den Werken von ARCHIMEDES. Waren es lateinische Uebersetzungen des XII-XIII. Jahrhunderts (64), die ihm vorlagen, oder kannte er die griechischen Originale ?

Weniger auffallend ist die ausgiebige Benutzung der Geographie von PTOLEMÄUS durch FONTANA im IV. Buche des *Liber*, da ja das gewaltige Werk des Alexandriners seit 1410 in der Uebersetzung von GIACOMO D'ANGELO DA SCARPARIA (JACOBUS ANGELUS) den Lateinern zugänglich war (65). Immerhin ist es interessant zu sehen, dass auch die Uebersetzungstätigkeit der Humanisten auf den sonst dem Mittelalter zuzurechnenden Venediger Arzt eingewirkt hat. FONTANA schätzt das Werk sehr

(64) Ueber die — heutzutage verschollenen — mittelalterlichen Uebersetzungen der *Pneumatik* HERONS vgl. A. BIRKENMAJER, *Vermischte Untersuchungen zur Geschichte der mittelalterlichen Philosophie*, Münster i.W. 1922, S. 22-30; die Werke von ARCHIMEDES wurden, wie bekannt, im J. 1269 von WILHELM VON MOERBEKE ins Lateinische übertragen, doch fand diese Uebersetzung nur sehr geringe Verbreitung.

(65) G. VOIGT, *Die Wiederbelebung des classischen Alterthums*, 3-te Aufl., II, Berlin 1893, S. 21, 173.

hoch (66), unterscheidet jedoch seinen Verfasser von dem Astronomen PTOLEMÄUS (67).

Zuletzt noch ein paar Worte darüber, inwieweit FONTANA den wertvollen cod. Paris. lat. 9335 (68) für seine Schriften benutzt hat. Dass die Handschrift im XV. Jahrhundert fleissig studiert worden ist, hat bereits BJÖRNBO bemerkt (69), und zwar auf Grund von Randglossen, welche die Handschrift selbst aufweist. Diese Marginalien sind besonders zahlreich an den Rändern von Nr. 11, 16, 17, 18, 21 und 26 des von BJÖRNBO veröffentlichten Inhaltsverzeichnisses. Rühren sie jedoch tatsächlich von FONTANA her? M.E. ist diese Frage zu verneinen, vielmehr unterscheide ich in diesen Randglossen drei verschiedene Hände des XV-XVI. Jahrhunderts und wage nicht zu entscheiden, ob eine derselben diejenige von FONTANA sei. Es fällt auf, dass die Abhandlung von ALHAZEN, welche von FONTANA zweifellos gründlich durchstudiert worden ist, keine Randglossen aufweist (abgesehen von derjenigen auf fo. 88*r*). Dies ist auch der Fall für die Abhandlung *De motu octavae sphaerae* von THĀBIT IBN QURRA (Nr. 24), welche ja in dem *Liber de omnibus rebus naturalibus* angeführt wird (70). Dasselbe gilt endlich auch für die *Sphärik* von THEODOSIOS (Nr. 1), obgleich auch dieses Werk in dem *Liber* ausgiebig benutzt worden ist (71) — und zwar in der Uebersetzung GERHARDS VON CREMONA, wie dies die angewandte Terminologie (z.B. *menguar vel axis, secare in duo media*) verrät. Auf jeden Fall kann also das Fehlen von Randglossen im codex Parisinus den Umstand nicht beweisen, der betreffende Teil der Handschrift habe für FONTANA wenig Interesse dargeboten.

20.VIII.1931.

Krakau (Polen). ALEXANDER BIRKENMAJER.

(66) Lib. IV cap. 8 (ed. 1544 fo. 94*r*) : « Nominem hactenus legi, qui clarius copiosiusque dixerit et magistraliter magis atque verius, quam CLAUDIUS PTOLOMEUS ALEXANDRINUS ».

(67) Lib. IV cap. 11 (ed. 1544 fo. 96*r*) : « ... nec CLAUDIUS PTOLOMEUS cosmographus, qui alius fuit ab eo, qui *Almagesti* pro theoricis planetarum atque stellarum condidit ». Von dem letztgenannten Werke benutzt FONTANA die arabisch-lateinische Uebersetzung.

(68) Gelegentlich bemerke ich, dass diese Handschrift m.E. im XIII. Jahrhundert, und nicht erst im XIV. geschrieben worden ist.

(69) Vgl. oben Anm. 52.

(70) Ed. 1544 fo. 34*v*. Vgl. THORNDIKE in *Isis* XV, 1931, S. 42.

(71) Ed. 1544 fo. 9*r*, 25*r*, 99*r*.

A Mathematical Text by Mordecai Comtino

(CONSTANTINOPLE, XV CENTURY)

Comtino's Time and Place. —

Rabbi MORDECAI COMTINO (1402-1482) (1), philosopher, mathematician, astronomer, exegete and poet, was one of a number of Byzantine religious leaders and teachers who during the XIV and XV centuries were engaged in literary and philosophical work and also in the exact sciences—mathematics and astronomy. This group includes, to mention only those to whom further reference will be made later, the following: the monk MAXIMUS PLANUDES (beginning of XIV century) whose book on Hindu (modern) arithmetic is one of the first treatises on the subject in Greek (2); IOANNES PEDIASIMUS, keeper of the seal to the patriarch of Constantinople (c. 1330), whose Heronian compilation on geometry resembles greatly the Geometry of COMTINO in the text under consideration; the monk ISAAC ARGYRUS (before 1368) who also wrote a Heronian geometry like the two just mentioned, and against whose unfavorable criticisms of the Jezdegerd Astronomical Tables COMTINO wrote a counter criticism (3); and lastly, COMTINO's two pupils, CALEB AFENDOPOULO (1455-1509+), teacher and Ḥacham of the Karaitic community of Constantinople, and prolific writer on various subjects; and ELIJAH MIZRAḤI (1455-1525), teacher and later

(1) The dates are learned from a colophon in MS. Bodl. 2774[5].

(2) Cf. T. L. HEATH, *A History of Greek Mathematics*, II, 547.

(3) *Perush Luhoth Paras* (MS. Petersburg 359)—a commentary on Persian Tables. In the Preface (quoted by JONAS GURLAND, *Ginzei Yisrael*, III, p. 2, Heb.) the author explains that it is his intention in this book to defend these tables against the criticisms of « Aryeru (Argyrus) and his school ».

Chief Rabbi of Constantinople who is known in the history of mathematics for his Arithmetic, one of the best texts written at that time (4). Thus Byzantine mathematics and astronomy which after ARGYRUS hardly showed any signs of life in Greek, continued their existence, in Hebrew garb, in the works of COMTINO and his pupils. With COMTINO's school the last spark of Byzantine science, (which was resuscitated some two centuries earlier, i.e. after the liberation of Byzantium from the yoke of her "brethren" the Latin Crusaders in 1261), seems to have flickered out. External and internal conditions during the XVI century, in spite of economic prosperity and political liberty that the Jews enjoyed under Turkish rule at that time, were no longer conducive to scientific pursuit. Externally there was hardly any stimulus, the Turks having annihilated whatever was left of Hellenic culture in Byzantium. Internally, the Cabbalah, imported from Spain at the end of the XV century, held such sway over the minds of Jewish scholars that interest in secular learning vanished almost entirely. This is clearly seen from the fact that out of nearly three hundred Hebrew books that were printed in Turkey during that century—(i.e. from the installation there of the first printing press (1493 or 1503) up to 1600 (5)—only one deals with a secular subject, and that is the above mentioned arithmetic of ELIJAH MIZRAḤI.

Returning to COMTINO, there have come down to us sixteen titles of his writings, nine of which consist of commentaries on the Bible, ARISTOTLE's Logic, and BEN-EZRA's writings; five deal with astronomical tables and instruments, and two with mathematical topics, viz., (1) Glosses to EUCLID, and (2) the text which is the subject of this paper, the *Sefer haḥeshbon v'hammiddoth*, or Book on Computation and Mensuration (6).

(4) For a detailed description of this Arithmetic, which will be frequently mentioned in the following, see GUSTAV WERTHEIM, *Die Arithmetic des Elia Misrachi*, 1893; 2. verb. Aufl., 1896.

(5) According to compilations by PORGES, FREIMANN and MARX in ROSANES' *History of the Jews in Turkey* (Hebrew).

(6) MSS. of COMTINO's Arithmetic and Geometry are registered in the Catalogues of the Libraries of Berlin, Paris, Leningrad, the British Museum, the Bodleian, and the Jewish Theological Seminary, New York (Adler Collection, No. 1576). The writer made most use of the last named MS., which is probably the oldest of them all. It was written according to its colophon, in 1478 for CALEB AFEN-

The writing of glosses and scholia or logistics and geodaisias forms the main line of Byzantine mathematical activity. COMTINO's writings are, therefore, typical of his time and place. This general lack of originality caused the products of the Byzantine School to remain scientifically unattractive and historically but little investigated. Historical research in this field is, however, a great desideratum. For, as pointed out by HEIBERG (7), many important problems in the history of Greek and medieval mathematics cannot be solved correctly without due consideration of the spiritual rôle that Byzantium played in the world and the various influences that played their rôle in Byzantium.

Comtino's Sources. —

The following sources were made use of by COMTINO in writing his book : Part I, the Arithmetic, is based largely on Rabbi ABRAHAM BEN-EZRA's *Sefer Hammispar* (Book of Number) which is the first algorism in the Hebrew tongue and one of the first in Europe (written shortly before 1160) (8). Besides BEN-EZRA, COMTINO mentions in his Preface NICOMACHUS GERASENUS' *Ἀριθμητικῆς εἰσαγωγῆς*. This medieval classic of arithmetic was still highly in vogue in XV century Byzantium, and one of COMTINO's pupils, the above mentioned CALEB AFENDOPOULO, wrote a commentary on it. COMTINO had little occasion, however, to make use in his book, a practical arithmetic, of NICOMACHUS' number theory and philosophy. In Part II, the Geometry, EUCLID is quoted about a dozen times. COMTINO's Geometry, however, is not Euclidean in origin, but Heronian; i.e., it is not a *γεωμετρία* but a *γεωδαισία à la Héron*. Besides the Greek works of HERON, COMTINO had before him also a Hebrew practical geometry *(Ḥibbur ham'shiḥah v'hattishboreth)* by Rabbi ABRAHAM BAR (son of)

DOPOULO, the above mentioned pupil of COMTINO, who filled some of the margins with his notes. Of its original eighty folios (21 × 15 cm.), two, f. 1 and f. 64, are missing. These lacunae the writer was able to fill out from two recent transcripts of other MSS. He was also aided by M. SILBERBERG's detailed description of the Berlin MS. in *Jahrbuch der jüd.-lit. Gesellschaft*, 1905, 1906.

(7) Byzantinische Analekten, *Abhandlungen zur Geschichte der Mathematik*, IX, 163.

(8) M. STEINSCHNEIDER, *Abraham ibn Esra, Zeitschrift f. Math. u. Physik*, 1880, p. 105 f.

Ḥiyya ha-Nasy (the Prince), commonly known as Savasorda, who flourished in Barcelona at the beginning of the XII century. Savasorda's Geometry is of considerable interest to the history of mathematics as it is one of the first mathematical texts to be translated from an oriental language into Latin (9). One century after Savasorda, Leonardo Pisano (1220) made this translation the basis of his *Practica geometriae*, following it, without acknowledgment, even to the sameness of some of the examples (10). Two and a half centuries after Leonardo, Comtino also incorporated in his book whole paragraphs from Savasorda's treatise, and again without mentioning either the name of Savasorda's treatise, or that of his other major source, Heron of Alexandria. It seems fairly certain, however, that in Comtino's case at least, the fault was not entirely his, but was due to two accidental literary errors. Thus, in the first place, his failure to mention Heron is explained by a misleading superscription on the manuscript copy that he happened to use. A comparison of the Heronian passages in Comtino with the various versions of the extant Heron MSS. revealed to this writer the fact that Comtino made use of the very same MS. (Codex Constantinopolitanus, XI century) which was discovered by Schöne in 1896 and published in 1903. This MS. contains, besides a few minor writings, Heron's *Geometrica*, *Stereometrica*, and a unique complete copy of his *Metrica*. Comtino borrowed freely from each of these three books (11). It happens, however, that the title « *Ἥρωνος εἰσαγωγαὶ τῶν γεωμετρουμένων* », which is found in other MSS. (12) of the *Geometrica*, is missing in Cod. Cpolitanus. (fol. 4^{r}). Instead there appears

(9) The translation entitled Liber embadorum was completed by Plato Tibertinus, with the aid of the author, on August 13, 1145. See C. H. Haskins, *Studies in the History of Medieval Science*, p. 11.

(10) M. Curtze, *Abhandlungen zur Geschichte der Mathematik*, XII, 5 f.

(11) The Heronian passages in Comtino's book follow this Cod. Cpolitanus in every detail, including all its errors and excluding all the occasional small lacunae that occur in it. Even the diagrams are the same in both texts. It may be interesting to add that this discovery by Schöne of Heron's authentic work (the *Metrica*) was considered by Tannery as the most important single contribution to the history of Greek mathematics made during the last two hundred years (P. Tannery, *Mémoires Scientifiques*, III, 131).

(12) MSS. A, C, et V (cf. *Heronis Alexandrini Opera*, vol. IV, p. 176, line 14, note).

on fol. 3[r] a superscription by a XV century reader : *Εὐκλείδου γεωμετρία* (13). This erroneous title led COMTINO to believe that he had before him one of EUCLID's treatises. He thus quotes (in the first Chapter of his Geometry) from « a book by Euclid on geodesy », a passage which is in fact a literal translation from folios 4-6 of this MS. (ed. HEIBERG, vol. IV, from p. 176, line 15 to p. 180, line 23).

His omission of SAVASORDA's name was probably due to a similar accidental error. Because of their homonymity Christian writers frequently interchanged ABRAHAM BAR ḤIYYA, the Prince, with his junior compatriot, ABRAHAM BEN-EZRA, « the Pauper » (14), just as the author of Almagest was frequently confused in medieval Hebrew literature with his homonym, King PTOLEMAEUS (15). COMTINO, too, evidently confused the two ABRAHAM's ascribing to ABRAHAM BEN-EZRA, to whom he acknowledges his great indebtedness on several occasions, the authorship of the Practical Geometry before him.

In his Preface COMTINO states that he followed « the Christian brief way of expression ». Further on, in Chapter II, he refers to cross multiplication as « a method I found among the wise of the Ishmaelites » (the Arabs). In another book of his (16), COMTINO makes mention of the oldest known Hebrew text on geometry, « Mishnath Hammiddoth ». There is also extant a MS. containing an arithmetic and a geometry by ISAAC BEN MOSES OF ARAGON which, according to its colophon, was copied for COMTINO (17). His main sources however, were, as stated above, HERON, ABRAHAM BAR ḤIYYA (SAVASORDA), and ABRAHAM BEN-EZRA.

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(13) Ibid., vol. III, pp. IX et XI ; vol. IV, pp. XII, et 173, note 1 et vol. V, pp. XXIII-XXIV.

(14) M. STEINSCHNEIDER, ABRAHAM JUDAEUS, *Zeitschrift f. Math. u. Physik*, 1867, p. 2.

(15) E.g., BEN-EZRA in his above mentioned *Sefer Hammispar*, ed. SILBERBERG, p. 45; v. note 101.

(16) A commentary on BEN-EZRA's *Sefer Hashem* (MS. Paris 681, fol. 7v); v. *Wiss. Zeitschrift f. jüd. Theologie*, 1844, p. 457.

(17) M. GRÜNWALD, *Jüd. Literaturblatt*, XXIII, 176. COMTINO's « cross-multiplication according to the Arabs » is mentioned also in this arithmetic, MS. Leyden (STEINSCHNEIDER, *Cat. Lugd.-Bat.*, p. 283.)

The Arithmetic

Notation.—Comtino while writing his Arithmetic had before him a choice of four or five different styles of numerals. Abraham Ben-Ezra, his major source, had combined the principle of place value with alphabetic numeration, using the first nine letters of the Hebrew alphabet to represent the digits 1-9 respectively, and a « wheel », ō, (18) for zero. A similar system was current among the Byzantines of Comtino's time who used as digits the first nine letters of the Greek alphabet with the symbol « ϙ » for zero (19). The influence of Persian astronomical works caused on the other hand the adoption by many Byzantine writers of Persian numerals with place value, as is shown, for instance, in the Hindu Arithmetic of the above mentioned Maximus Planudes, in the Scholium of the monk Neophitos (20), and in Cod. Marcianus Gr. 323 (21). In the latter two the zeros are written above the significant figures, thus $\overset{0}{21}=21$ $\overset{000}{1}=1000$. Others, like Nicholas Rhabdas (22) (1341) and his contemporary, Barlaam (23), adhered to the old system of alphabetic numeration without place value. Greek MSS. prior to the XVI century show thus, according to Paul Tannery (24), no less than four varieties of numeral forms, two oriental and two occidental. Comtino wrote his book sometime before 1478. He must, therefore, have had before him these various systems besides the Ben-Ezra combination of Hebrew characters with place value which was adopted also by the philosopher and mathematician Gersonides (25) (1288-1344) and the above men-

(18) ō with a bar on top occurs in many old Ben-Ezra MSS. and also in MS. Adler of Comtino. *Vide* Steinschneider, Abraham ibn Esra, p. 114; Smith-Karpinski, *The Hindu-Arabic Numerals*, pp. 61, 143 and 148 ; F. Cajori, *A History of Mathematical Notations*, I, pp. 28 and 51.

(19) J. L. Heiberg, *Byzantinische Analekten*, p. 164.

(20) Tannery, *op. cit.*, vol. IV, p. 20 ff.

(21) Heiberg, *op. cit.*, p. 173 f.

(22) Tannery, *op. cit.*, p. 79.

(23) Barlaam wrote a logistic in six books using throughout Greek letters for numerals. It was printed at Strassburg as late as 1564 and reappeared in several later editions (F. Cajori, *op. cit.*, I, 26).

(24) *Op. cit.*, p. 205.

(25) « *Ma'aseh Ḥosheb* » (ed. Lange).

tioned MIZRAḤI. Of all these COMTINO decided on the Hindu (modern) numerals, expressing particularly his objection to the double use of alphabetic numerals, namely with and without place value (26).

In connection with this discussion COMTINO expresses another opinion which is contrary to that of his « master », BEN-EZRA, and his pupil, MIZRAḤI. It is concerning the naturalness of the decimal system. BEN-EZRA found a quasi « geometric proof » for our number system being of necessity decimal. MIZRAḤI amplified this with another proof, an « arithmetical » one. COMTINO, however, passed over the subject by briefly remarking that the decimal system seems to him to be simply a human conventionality.

All computations with figures in *Sefer Haḥeshbon* are done in modern numerals, COMTINO being perhaps the first to introduce this practice into a Hebrew text. The novelty, however, presented a dilemma to the copyist, namely : which is the correct way of writing numerals in a Hebrew text, from left to right or the reverse? The first form is un-Semetic, the second—unaccepted even in Arabic. Being at a loss, SHABBETHAI BEN MOSES, the copyist of MS. Adler, wrote them sometimes one way and sometimes—the other. We thus find in this MS. such forms as 0027 for 7200, $\frac{01}{01}$ for $\frac{10}{10}$, or ō ō21 42 ō5 for the proportion 50 : 24 = 120 : x, (ō stands for the unknown) (27). (Similar confusion is met with in modern publications of music set to Hebrew or Arabic words.)

The Four Operations.—Next to notation the operations and their sequence are of interest. BEN-EZRA, COMTINO's model, teaches multiplication before addition and subtraction. So did also SAVASORDA in his Encyclopedia (c. 1120), LEONARDO PISANO in his *Liber abaci* (1202), and as late as COMTINO's time, PIETRO BORGHI in his well known Arithmetic. COMTINO gives the four

(26) In their verbal explanations COMTINO, as well as BEN-EZRA, GERSONIDES and MIZRAḤI, retained the Hebrew alphabetic numerals without place value. Hence, COMTINO's objection to using the same alphabetic characters also with place value.

(27) This old, originally Hindu, usage of o to represent the unknown quantity (since o means the missing of a number), was apparently still popular in COMTINO's times. It occurs, e.g., also in a contemporaneous Latin manuscript of lectures given at the University of Erfurt (v. SMITH-KARPINSKY, *op. cit.*, p. 54).

fundamental operations in their modern sequence, his procedure with each differing but little from the one now commonly in use. Duplation and mediation, although given as distinct operations in one of COMTINO's (probable) sources (28), are not mentioned by him. Multiplication and division are each taught by two methods : the ones now commonly used and also cross multiplication which COMTINO describes as « an easy method borrowed from Arab scholars », and the « galley method of division which is used by a majority of merchants ». (COMTINO, a Jew, would evidently not use a name like « cross » multiplication as did PLANUDES (29), but instead he and MIZRAḤI refer to this method as « scissors-like multiplication »). Oral multiplication being no less important than written multiplication, COMTINO gives a set of formulae (in words) to facilitate this practice. These formulae are nothing but the simple identities :

$$a^2=(\tfrac{1}{3}a)^2 .\ 10\ -(\tfrac{1}{3}a)^2,$$
$$a^2=(\tfrac{1}{5}a)^2\ .\ 10.2\tfrac{1}{2},$$

which COMTINO (and also BEN-EZRA, GERSONIDES and MIZRAḤI) particularized according to $a = 3n$, $3n\pm1$, $5n$, $5n\pm1$, or $5n\pm2$. An arithmetical paraphrase of the first ten Propositions of Book II of EUCLID's Elements concludes the exercises on multiplication.

COMTINO illustrates his method of cross-multiplication and galley-division by three examples, two of which will be rendered here, with the substitution, however, of figures instead of the verbal explanations.

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(28) The above mentioned Arithmetic of ISAAC BEN MOSES; cf. STEINSCHNEIDER, *Abraham ibn Ezra*, p. 112.

(29) κατὰ χιασμόν = over the cross, *Ψηφοφορία κατ' Ἰνδούς*, ed. GERHARDT, p. 10.

Cross Multiplication

Example. Multiply 235 by 348.

$8 \times 5 = 40$ 0

$8 \times 3 = 24$ (4)
$4 \times 5 = 20$ 8

$8 \times 2 = 16$ (4)
$3 \times 5 = 15$ 7
$4 \times 3 = 12$

$4 \times 2 = 8$ (4)
$3 \times 3 = 9$ 1

$3 \times 2 = 6$ (2) 8

81780

GALLEY method of division as explained step by step in *Sefer Haḥeshbon*.

Example. Divide 104034 by 114.

I.	104034 114	9	II.	1 104034 114	9
III.	15 104034 114	9	IV.	1 154 104034 114	9
V.	1 154 104034 1144 11	91	VI.	1 154 104034 1144 11	91
VII.	13 154 104034 1144 11	91	VIII.	2 13 1549 104034 1144 11	91
IX.	2 13 1549 104034 11444 111 1	912	X.	2 13 1549 104034 11444 111 1	912
XI.	2 137 1549 104034 11444 111 1	912	XII.	26 137 15496 104034 11444 111 1	912

That is, 104034 : 114 = 912, with a remainder of 66. (The remainder is indicated by the two non-cancelled figures in the upper right of XII.)

After each operation COMTINO gives two checks which like BEN-EZRA (and FIBONACCI) he calls « balance ». One is by casting out the 9's; the other by doing the same with the 7's. Both

of these checks are given also by MIZRAḤI, but only the first of them is given by BEN-EZRA and PLANUDES. A more important distinction between the latter and the former is that while BEN-EZRA and PLANUDES evidently believed that the check was a complete one, COMTINO and MIZRAḤI recognized its limitations. COMTINO, therefore, gave a third check, or « just balance », namely, the performing of the inverse operation.

Fractions.—A fraction is defined by COMTINO as one or more equal parts of a unit. BEN-EZRA and MIZRAḤI define it similarly. The more scientific GERSONIDES based his definition on division of a number by a non-aliquot divisor. The numerator is usually referred to by these writers as « the number » (of parts) ; the denominator, as the indicator (moréh). COMTINO used also the term *yesod* (= basis) to designate the denominator (30). MIZRAḤI employs the very suggestive terms *kammuth* (quantity) and *eikhuth* (quality) to designate numerator and denominator respectively.

Following Arabic writers, the early Hebrew arithmeticians classified fractions as either expressible or non-expressible ones (31). This classification had nothing to do with the nature of fractions per se, but simply with a grammatical peculiarity of the Arabic and Hebrew languages, namely : the absence of the ordinal form for numbers greater than ten, and consequently the lack of names for parts other than 1/2, 1/3,..., 1/10. The latter fractions were therefore called expressible. Fractions whose denominators are greater than ten were reduced, whenever possible, to the expressible type by factorization. BEN-EZRA thus writes « one fifth of an eight of a seventh » for 1/280, and in GERSONIDES such expressions as « five eights of a seventh of a third of a third » for 5/504 are not uncommon (32). In cases, however, like 13/19 where factorization is impossible, they had to use the circumlocutory phrase « thirteen parts of nineteen », and characterized such fractions as dumb or non-expressible. COMTINO, following HERON, made also frequent use of unit fractions ; 20/21, for instance, he expresses as 1/2 + 1/3 + 1/12 + 1/28.

(30) Cf. D. E. SMITH, *History of Mathematics* II, 220.

(31) SILBERBERG, *Abraham ibn Esra's Sefer Hammispar*, p. 104, note 78.

(32) V. BOBININ (*Bib. Math.*, 1889, p. 105) tells about similar curiosities in old Russian, but there they are due to different reasons.

In the mode of writing fractions the mentioned Hebrew authors differed considerably. GERSONIDES wrote out his common fractions in words and not in figures. His writing and treatment of sexagesimal fractions, however, resemble greatly our decimal fractions. As a separatrix between the integers and the fractions he used a perpendicular bar. From this to the invention of decimal fractions there was only one step. It took, however, two centuries before this step was taken.

BEN-EZRA's written (common) fractions are in Moorish figures, usually without the dividing bar. COMTINO's fractions are in modern numerals, with the bar between, but as stated above, the figures are arranged from right to left ($\frac{01}{01}$ for $\frac{10}{10}$). He does not, as did many of his contemporaries, avoid the use of improper fractions, but he does not, on the other hand, reduce his fractions to the lowest terms. MIZRAḤI frequently follows COMTINO, but rarely his mistakes. MIZRAḤI's fractions are entirely modern, written from left to right and reduced, whenever possible, to their lowest terms.

COMTINO considers in order multiplication, addition, division and subtraction of fractions. Following BEN-EZRA, the fractions in all four operations are first reduced to a common denominator, usually the product of the given denominators. In addition, subtraction and division, the sum, difference or quotient, respectively, of the new numerators is taken. In multiplication, however, this led them to the curious plan of dividing the product of the new numerators by the product of the new denominators. Thus in the case of $3/4 \times 4/9$ they have,

$$3/4 \times 4/9 = 27/36 \times 16/36 = 432/1296 = 1/3.$$

This plan, they admit, is too cumbersome, and another method is therefore given, the one now commonly used.

Proportion. Exercises.—A considerable part of the *Sefer Haḥeshbon* is devoted to the application of the geometric proportion $a : b = c : d$, commonly known and designated also by COMTINO as the Rule of Three. Here, too, BEN-EZRA's influence is felt, but the treatment of the subject as a whole is largely Byzantine. To the Byzantines, this was a rule of the highest importance in the arithmetic of daily life. The above mentioned NICHOLAS RHABDAS, for example, calls the application of this rule πολιτικὸς

λογαριασμός, political arithmetic, or computation of civil life (33). Another Byzantine, a contemporary of COMTINO's, names this rule « the soothsayer of arithmetic » (34). COMTINO, too, treats the subject in considerable detail. He divides it into two parts, one dealing with integers and the other with fractions, assuming, as he states, that there is a different method for each kind.

Simple proportion is followed by double proportion, or Rule of Five, of which the following is an example.

Problem. If 2/4 of a denier earned 3/4 of a denier in 4/8 of a day, i.e., in twelve hours, how much will 4/4 of a denier earn in 4/8 of a day? The answer is arrived at by the following successive steps :

$$\begin{aligned} x : 4/8.4/4 &= 3/4:4/8.2/4, \\ \therefore\ x : 4/8.4/4 &= 3/4:8/32, \\ \therefore\ x : 4.8.4/4 &= 96/32, \\ \therefore\ x : 4/8 &= 384/128, \\ \therefore\ x &= 1536/1024 = 1 + 1/2. \end{aligned}$$

COMTINO closes his *Sefer Haḥeshbon* with a set of twenty-one questions and answers. Eighteen of these he drew from his major source, BEN-EZRA's *Sefer Hammispar*, while the remaining three, although long and well known in Mohammedan and Christian arithmetics, seem to have made their first appearance in Hebrew in this text. One of these three is the ubiquitous Heronian problem relating to pipes filling a cistern (35). The second is an application of the Pythagorean Theorem, resembling the type of problems found in AL-KARKHI's *Kafi fi'l ḥisab* (36) and in *Liber abaci* (37). The third leads to an indeterminate equation of the first degree (a fact which the author failed to recognize), and is much like the problem proposed by Magister MUSCUS OF CONSTANTINOPLE in his challenge to LEONARDO PISANO (38). Barring this last one, all the other problems lead

(33) TANNERY, *op. cit.*, pp. 13, 79 et 140.

(34) HEIBERG, *op. cit.*, p. 167.

(35) D. E. SMITH, On the Origin of Certain Typical Problems, *American Mathematical Monthly*, 1917, p. 64 f.

(36) Ed. HOCHHEIM, III, 16, 26.

(37) *Scritti di Leonardo Pisano*, I, pp. 331, 398, 240 et. seq.

(38) *Ibid.*, p. 249. Cf. M. CANTOR, *Vorlesungen* (2), II, 24-25; J. CARLEBACH, *Lewi ben Gerson als Mathematiker*, p. 219.

to simple equations with one unknown. Both in form and content COMTINO's problems and methods of solution remind one of a similar set of (18) questions and answers given by RHABDAS in his logistical letters (39). The problems in both texts are put in the form of little stories and the solutions consist of dogmatic instructions which, perhaps due to a fault of the copyists, are not always correct.

Nearly all of COMTINO's examples are incorporated, with some modifications, in a set of 100 problems appended to the arithmetic of COMTINO's pupil, MIZRAḤI. There is a vast difference, however, between the methods of the teacher and the pupil. In the first place, COMTINO, like most Byzantines, merely delivered what he took from others, while some of MIZRAḤI's methods were entirely original with him (40). Then again, as pointed out before, COMTINO merely gave rules which he only very infrequently proved, while MIZRAḤI proved, or at least endeavoured to explain fully, every rule and process he taught. For, unlike most of his contemporaries and many later writers, MIZRAḤI clearly recognized the pedagogical principle, namely: that the training of mathematical reasoning ability is far superior to the mere acquisition of rules. His own statement to this effect (written ca. 1500) furnishes an interesting document in the history of pedagogy, and an excerpt of it shall, therefore, be quoted here:

Says MIZRAḤI (41): « All the ancients whose works on this subject (arithmetic) reached us endeavoured, it is true, to instruct the methods and processes by means of which the required results are attained; but their intention was mainly to shorten the methods leading to the results, and nothing else. Their methods, therefore,

(39) Cf. TANNERY, *op. cit.*, pp. 13-15 et 79.

(40) In his Preface MIZRAḤI states that « in this book are found new processes, which I invented, and important artifices ». It may be remarked in this connection that contrary to the opinion of STEINSCHNEIDER and several others (*Jewish Encyclopedia*, Art. *Mizraḥi*; G. WERTHEIM, *Die Arithmetik des Elia Misrachi*, p. 5; SMITH, *History*, I, 350), MIZRAḤI was not the first Hebrew mathematician to treat of the summation of the cubes of the first n natural numbers, since the summation formula $\sum_1^n n^3 = (\sum_1^n n)^2$ is found already in GERSONIDES' Arithmetic (« *Ma'aseh Ḥosheb*, ed. LANGE, p. 62; cf. Preface, p. XI) which was written nearly two centuries before that of MIZRAḤI.

(41) Preface to his *Sefer Hammispar*, Constantinople, 1534 (published posthumously; v. note 4 above).

looked to the students like an uninterpreted dream, because they did not understand how, nor why, the results followed from the method. They merely groped the wall like blind men. Also, not knowing the reasons for the methods, they could follow only that author whose methods they happened to practice. The ways of other authors were to them ' straitly shut up, no going out nor coming in '; not to mention that they were not able to devise any method of their own of proving a theorem or solving a problem unless the latter happened to resemble one of the problems that they had already encountered previously. But, as you well know, the main thing in this science is the solving of problems, as it is this that leads us into its deepest investigations ».

Notwithstanding this just criticism against his forerunners, COMTINO included, MIZRAḤI nevertheless owed a debt of gratitude to the latter whose *Sefer Haḥeshbon* served him as a source in several instances.

THE GEOMETRY

The Geometry, the second part of COMTINO's book, treats of mensuration of the square, rhombus, triangle, trapezium, the circle and its segments, regular polygons (from the pentagon to the dodecagon), parallelepiped, prism, cylinder, pyramid, cone and frusta, sphere and segments, and also of the division of plane and solid figures into equal parts. The general arrangement as well as the treatment of the subject-matter is largely that of the above mentioned Geometry of SAVASORDA. But, like PEDIASIMUS and ARGYRUS, COMTINO incorporates also a good deal of Heronian matter in his treatise (e.g. the chapters on trapezia, regular polygons, and pyramids). The Hebrew material that he took from SAVASORDA COMTINO usually paraphrased and abridged; the Greek of HERON, on the other hand, he simply translated literally, including as stated above (11), all the errors found in SCHÖNE's manuscript which he evidently used, and excluding the occasional small lacunae that occur there. The correct as well as the incorrect rules found in this book can thus be mostly traced back to either HERON or SAVASORDA, and need not be mentioned further. A few errors, however, that were overlooked by the editors of the works of HERON and SAVASORDA will be pointed out here.

HERON (42) and, following him, COMTINO found the lateral area of a cone of revolution according to a formula : lat. area = $1/2.\pi ad$, where $\pi \sim 22/7$, d = diameter of circular base, and a = altitude of cone. Here HERON, or the PSEUDO-HERON, confused the altitude with the slant height.

Again, HERON (43) and COMTINO state that the vertical height of a pyramid whose base is an isosceles triangle is equal to $\sqrt{e^2-(a/2)^2}$, where e = edge of the pyramid, and a = altitude of the isosceles triangle (on its base). To this HEIBERG (43) remarks : « It is evidently assumed here that the altitude of the pyramid bisects the height of the triangle ; this is not true generally, except in the case when the triangle at the base is equilateral. » The fact, however, is that it is not true even in this case.

COMTINO gives the following two formulae for a spherical segment :

$$A = \pi da,$$
$$V = 1/6Ad,$$

(A = spherical surface of segment, a = its altitude, d = diameter of sphere, and V = volume of segment).

The first formula is correct, and is derived from ARCHIMEDES(44). The second holds true for a spherical sector, but not for a segment. COMTINO « borrowed » this error from SAVASORDA (45) as did another XV century mathematician, SIMON BEN ṢEMAḤ DURAN (46). Strangely, neither GUTMANN, the editor of SAVASORDA's Hebrew work, nor CURTZE (47), the editor of its Latin translation, took cognizance of this error.

A TELLTALE NOTE

On the last page of MS. Adler (6) there is an interesting note in minute letters which hitherto has escaped the notice of bibliographers. It reads : « Declaration, that I shall teach no man any of the mathematical sciences, viz., astronomy and geometry

(42) *Opera*, V, 18.
(43) *Op. cit.*, p. 146.
(44) J. TROPFKE, *Gesch. d. Elementar-Mathematik*, II, 392, (1903).
(45) *Ḥibbur ham'shihah v'hattishboreth*, ed. GUTMANN, p. 107.
(46) Quoted by J. GINSBURG, *Hadoar*, 1928.
(47) *Abhandlungen*, XII, 174.

either theoretical or applied, which I learned from my teacher R. ELIJAH BASHYAẒI as long as he lives, unless he gives me permission to do it. May God grant him long life, amen ».

The anonymous writer was, as seems from a comparison of handwritings, the first owner of the MS., CALEB AFENDOPOULO, pupil and brother-in-law of BASHYAẒI. This conjecture is corroborated by another note found in MS. Paris 1024 wherein AFENDOPOULO imposes a similar oath on one of his pupils, with the additional stipulation that the latter should in turn make his pupils pledge themselves likewise (48). This recalls to mind the famous Hippocratic oath administered to young physicians in ancient Greece, which also contained the promise : « I will impart the knowledge of my art to my sons and those of my masters and to disciples bound by a stipulation and oath—but to none others ». Among mathematicians, however, such a monopoly was never heard of before (49), and yet it was not at all uncommon in the XV century. Thus we find German cossists and Italian algebraists exacting pay for the solution of problems. One of the writers of this period woefully complains that « few know their letters; those who do, are eager to keep their knowledge to themselves, confiding it only to their pupils in their schools»(50). Oddly enough, a similar state of affairs prevailed then also in Japan. There, too, mathematicians kept their science secret, revealing it only to their trusted desciples (51). Aside from simple vanity, which evidently was the cause of this secrecy in some cases, it was mainly the dependence of the scholars on the precarious income derived from their pupils that compelled them to monopolize, or, more correctly, to copyright the subject matter of instruction.

PINCUS SCHUB.

(48) CARMOLY, *Israelitische Annalen*, 1839, p. 397.

(49) The secrecy practiced among the Pythagoreans was due to entirely different reasons.

(50) F. UNGER, *Die Methodik der practischen Arithmetik*, p. 33 f.

(51) YOSHIO MIKAMI, *Scientific Japan, Past and Present*, p. 185.

Notes on the work of Polydore Vergil "De Inventoribus Rerum"

BY

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The late JOHN FERGUSON, Regius Professor of Chemistry in Glasgow University from 1874 to 1915, delivered a lecture on the work of POLYDORE VERGIL and his « *History of Inventions* » in the year 1908 to the subsection of the History of Medicine and the Natural Sciences at the International Historical Congress at Berlin, Professor KARL SUDHOFF being in the chair as President of the Section.

The draft of this paper was left in manuscript and at his death passed into the possession of the Glasgow University Library along with his other works. It was thought that, although this paper was not finished in his usual careful style, and constituted more or less a rough pencil draft of his speech to the Society, still, with some revision, its interest and the research into the place VERGIL's *History* held, should not be lost.

Long before he gave this short résumé of POLYDORE VERGIL's Life and Works, he had contributed largely upon the same subject to the Archaeological Society of Glasgow in 1883 and 1884 (*Trans.*, Parts II, III) also to the Society of Antiquaries of London in 1888 upon *The English Translation of Polydore Vergil's « De Inventoribus Rerum »* published by the Society in *Archaeologia*. Later, in 1891, he read to the same Society a paper on *The Bibliography of Polydore Vergil*. By the time he delivered his address at Berlin he had collected more information regarding the Bibliography than any previous investigator. While BECKMANN cited only ninety six editions, Professor FERGUSON had enumerated and catalogued from

almost every library in Europe and Britain, one hundred and ten. This Bibliography, which is still unpublished and was added to, till within a year or two of his death, is a monumental work of profound research and minute and laborious transcription of titles and references to early commentators on the work.

In revising this manuscript I am greatly indebted for most kind and valuable help and collaboration from an old friend of mine and Professor FERGUSON, who is a bibliographer and a scientist.

The footnotes on bibliographical matters I have added myself, in order to be of use to those whose studies lie in this direction and who may wish to consult Professor FERGUSON's other writings on the same subject.

(Glasgow) 1931 ELIZABETH H. ALEXANDER.

INTRODUCTORY

The History of Science, whether as regards its theory, practice, or applications, attracts few readers and still fewer students and writers, as compared with other departments of human activity, such as the history of literature, politics, art or philosophy. The audience to which such a history should appeal is a limited one at the fullest, and almost an uninterested one. The student of a Science is more eager to know accurately the contemporary state of the Science itself which he prosecutes, than its past condition; there is little or nothing in its past history which specially concerns or assists him in his work. For the same reason the historians are still fewer—for if any one should know enough to write a history, the probability is that he would much rather engage in the work of the Science itself. To write a history requires the convergence of qualities not always found together, a knowledge and keen interest in the facts, the historical capacity, and the inclination to relinquish the study of nature for that of the history of the study. No man with strong impulse to investigation will trouble writing history, he will prefer to think that he is making it. The difference, in fact, lies in this : in the history of philosophy, or of literature, or of civil or ecclesiastical affairs, it is the work and thoughts of men that are studied; in science it is the phenomena of nature that are involved and men's

Isis, XVII, pl. 7. J. FERGUSON.

POLYDORE VERGIL
1470?-1555?

Portrait found among Professor FERGUSON's papers at Glasgow University, artist unknown.

thoughts about them are of interest only till the reality has been discovered and determined, and then the interest in the ideas as such ceases. There is no human idea in a law of material nature (humanly discovered). It is only when they have been handled anew by the historian and are correlated thereby with the other departments of historical—that is, human, not natural—investigation, that they acquire significance and permanent interest. The history of science therefore is not science itself—it is a branch of the history of philosophy, or in other words, it is an account of the progress of human thought directed towards the comprehension of the phenomena of nature. The comparative neglect of the subject hitherto is due to two causes : on the one hand it does not assist the scientific man in his work; on the other it does not appeal to the ordinary man, that is, the great majority of mankind, because it has no human interest for him. He is ignorant of the technicalities, their expression, and the terms, he knows nothing of its phenomena and he therefore brings nothing in response to the demands which its history makes upon him.

There is besides this difference between science and art or literature or politics that while in science there is a continual replacement of the old by the new, of the previous by the present, there is no superseding of a work of art, or of letters, or of philosophy, or of poetical work by anything that comes after. There are many literatures and many periods in them, many periods in art, many styles. There are varying political systems, but there is only one kind of mathematics or chemistry or physics, and the errors and defects and obscurities of older times are superseded actually and replaced by fuller and more accurate knowledge.

It is the permanence of periods in arts and literature which make their history attractive to the ordinary man—it is the superseding of one so-called fact, by a more accurate statement of the phenomenon, which does away with all interest in the previous phase in science. If a statement or description has been proved wrong and is corrected, it had better be put out of sight and forgotten. It has served its purpose and is now devoid of vitality. It is an obstacle to progress. The theory of the composition of metals from sulphur and mercury gave way to phlogiston,

phlogiston gave way to oxygen, what oxygen may have to give way to has not yet been settled. The ideas of former times have passed away and are useless. They make no appeal to the scientific men of the present time. They tell him of nothing but the evanescent character of his firmest beliefs. But one cannot say the same of the works of literature or of art. The *Nibelungenlied* has not been superseded by *Faust*, *Theuerdanck* (1) has not been replaced by the *Buch der Lieder*. The human element in these is the same and is always comprehensible and survives. MARCELLO has not been proved wrong by BACH, or PALESTRINA by SCHUBERT, or PURCELL by ELGAR or BANTOCK.

A Picture Gallery is prized for the works of the various schools —Dutch, French, Italian, Spanish—which it contains. But it is not so in science. There every thing is constantly in motion. In it we deal not with the creation of something new as in literature or art, but with the discovery of what exists and consequently with everchanging aspects of what has already been discovered. There are no Schools of Science as there are of painting. *Jurare in verba magistri* has no meaning in science, or if it has it denotes imperfect knowledge, a substitution of opinion, possibly acute opinion, for accurate knowledge. The only master—or in this case mistress—who has the last compelling word in every case is Dame Nature herself and all our *opinions* have to be amended or superseded when she allows more of her secrets to be revealed.

If what has now been said be correct as regards what is commonly called pure science, still more applicable is it to empirical arts and crafts and trades, even when they have been placed, as some have, on a scientific basis. To write a systematic history of these is almost an impossibility, from the extent of the matter and from its want of connection. It might be possible to compile a chronology of certain inventions, trades or crafts, showing when new methods or new tools or apparatus were introduced. Such epochs exist in all trades and manufactures, but that would hardly constitute their history, and would be entirely uninteresting.

Nevertheless the attempt has been made by more writers and over a longer period than one would suppose. Not to speak of PLINY who does not ignore invention and discovery, passing

(1) A romance attributed to MAXIMILIAN I.

by ISIDORUS, who has something to say on these themes also, and coming down to quite modern times, there is no lack of writers who have laboured to give a *vidimus* of invention—as distinguished from discovery.

One could enumerate well nigh a score of authors who have handled the subject from PASTREGICUS to BECKMANN and DUTENS and GOGNET and ROLLIN. They have worked in different ways, on different scales with different results. A number have dealt with the inventors, some at full length, others as briefly as possible, while the inventions were enumerated and the inventors discussed by others. Among the latter BECKMANN still remains preeminent for his vast reading, minute and critical acquaintance with details and the interest of his narrative. Yet even he confined himself to certain selected topics and did not attempt to cover the whole field of invention. But one still turns to his *Geschichte der Erfindungen* though it is now 120 years old, for information on many curious points of discovery. If it were then beyond even his power, as a professor of Technology, to write the history of all invention, it would be still more difficult for any single person to attempt the same task at the present day—the practical arts have become too great, too detailed for any one to write their history, and if such a thing were required, it would need to be dealt with by specialists in the several departments of applied science. A historical commission on Technology, but nothing short of that, might manage to produce a survey of the subject. This alone shows what progress has been made in four centuries, when one contrasts what could be done then with what would be required now.

At that early time there appeared a small volume written in Latin, printed rather crabbedly, which professed to deal with invention and inventors. It was the work of a Churchman, not of a Craftsman, written from a scholar's point of view, not from the workman's. Herein is evidence of what has been already stated, that the craftsman is too busy with his craft to think of writing its history, and he leaves that work to him whom it concerns. The scholar and churchman who found the subject of sufficient attraction to investigate it, was an Italian, POLIDORO VERGILIO—and here I may remark in passing that, as far as my knowledge goes, all the early writers on the subject were Italians,

PASTREGICO (2), MATTHAEUS LUNENSIS (3), SARDUS (4), PANCIROLI (5), ZACHARIUS LILIUS (6), SABELLICUS, CONTARENI, BRUNO DI MELFI, and others.

It is to VERGIL's (7) book, however, that the following remarks apply. Here, however, it may be convenient to say a few words about the author himself. Fortunately his biography is a very simple one. There are no chronological difficulties to contend with, the narrative is straightforward, and his biographers are agreed practically on all points.

BIOGRAPHY

The author, POLIDORO VERGILIO, an Italian, was born at Urbino, about 1470. His great grandfather had been professor of philosophy at Paris besides being skilled in medicine and astrology, his younger brother was professor of philosophy at Pavia, and he himself studied at Padua and Bologna, apparently with the greatest distinction as regards both scholarship and learning. In

(2) GULIELMUS PASTREGICUS, sometimes called PASTREGICO, PASTRENGUS, PASTREGIUS and PASTRENGO from the little village of Pastrengo where he was born early in the 14th century. His book *De Originibus Rerum Libellus*, Venice 1547, see Professor FERGUSON's *Notes on some Books of Receipts or so-called Secrets*, Part II. Transactions of the Archaeological Society of Glasgow.

(3) JOANNES MATTHAEUS LUNENSIS. Born at Luna, Etruria, died about 1520 and wrote *Libellus, De Rerum Inventoribus*, edited by GIUSTINIANI. See *Joannes Matthaeus and his Tract « De Rerum Inventoribus »* by Professor JOHN FERGUSON. Printed in the Proceedings of the Royal Philosophical Society of Glasgow, 1902, Part I; Part II printed in 1916.

(4) SARDUS, or SARDI ALESSANDRO, born at Ferrara 1520, died 1588. « His tract is entitled *De Rerum Inventoribus Ll.* 11. *iis maxime quorum nulla mentis est apud Polydorum, n. pr. in lucem editi*. It first appeared at Mayence 1577 and is not tue first edition of that work printed at Venice in 1557. It was afterwards printed with VERGIL's work and is contained in the 1604 and 1626 editions. » See Professor FERGUSON's *Notes on Books of Secrets*, Trans. Arch. Soc. of Glasgow, Part II.

(5) GUIDO PANCIROLLO, born at Reggio 1553, died 1599. His History appeared « exactly a century after POLYDORE VERGIL's. »

(6) ZACHARIUS LILIUS, « *De origine et Laudibus Scientiarum* », printed at Florence, 1496, see Professor FERGUSON's *Notes on Books of Secrets*, Trans. Arch. Soc. of Glasgow, Part III.

(7) *Bibliographical Notes on the English Translation of Polydore Vergil's work « De Inventoribus Rerum »*, communicated to the Society of Antiquaries by Professor JOHN FERGUSON, 1888. *Archaeologia*, Vol. LI, p. 35. *Notes on the Bibliography of Polydore Vergil*, Part II, Trans. of the Archaeological Society of Glasgow, 1883.

evidence of his diligence were the works which were the results of his studies there, viz., his preface to the edition of PEROTTUS's *Cornucopie*, a commentary on certain parts of MARTIAL, published in 1496, his book of *Proverbs* published at Venice in 1498 (which he himself calls the first fruits of his learning in his dedication of the book to the Duke of Urbino and which displays a very creditable amount of reading and reflection), and his three books *De Inventoribus Rerum*, Venice 1499, dedicated to ODAXIUS, all the earliest of their kind.

These works, rapidly succeeding each other, would have indicated the career of a scholar or professor for VERGIL, but instead of that he drifted away into public life; and though he never gave up study and writing, he had other important business to discharge.

He was made Chamberlain to Pope ALEXANDER VI, by whom he was sent to England about 1501 as subcollector of Peter's Pence, his superior in office and the treasurer of the fund being a kinsman of his own, ADRIAN DE CASTELLO, who had been created a Cardinal under the name of St. Chrysogoni. The latter became bishop of Hereford and in Nov. 1503 POLYDORE VERGIL was presented to the rectory of Church Langton in Leicestershire, and when the Cardinal in 1504 was translated to the Cathedral of Bath and Wells, he was enthroned in the person of VERGIL who acted as his proxy on the occasion.

In the following year, 1505, at the request of the King, HENRY VII, he began his History of England, at which he worked assiduously for 28 years, and had every facility given him in access to records and MSS. that he required. In 1507 and 1508 various church preferments were conferred upon him, as the prebend of Scamblesby in the Church of Lincoln, the Archdeaconry of Wells, and others, but he resided for the most part in London, corresponding with friends in Italy and living in friendly intimacy with the most notable scholars in London.

In 1510 he was naturalized and in 1513 he resigned Scamblesby and was collated to the prebend of Oxgate in St. Pauls.

The following year he fell into disfavour with WOLSEY, he was deprived of the subcollectorship and spent some months in prison. This excited a good deal of interest at Rome and at Oxford, and letters were sent to HENRY VIII, requesting that

he might be set at liberty. How long he was confined does not definitely appear, but he was free by December 1515, still retaining his benefices.

During the long time he was engaged with his History of England he did other literary work. He revised his History of Inventions and added five additional books. In 1524 he wrote a commentary on the Lord's Prayer which was often printed along with the book of inventions. In 1525 came his quaint little edition of GILDAS's History, which was the first publication of it from two MSS., and then in 1526 his tract *De Prodigiis*, the aim of which was to show that most of the prodigies could be explained on rational grounds, and that those which could not, did not exist. In 1528 he dedicated to ERASMUS a translation from the Greek of a fragment of ST. CHRYSOSTOM, which, as he says, was his return to the study of Greek, interrupted by his other labours.

The History of England, his chief work, was completed in 1533, and was printed at Basel in 1534 with a dedication to HENRY VIII. It forms a very handsome volume in folio.

He went afterwards to Italy on business and on his return his health seems to have failed. Anxiety as to the condition of affairs both civil and ecclesiastical may have been the occasion of this, for he was not what might be called an old man, but towards the year 1550 he asked leave to return to his native country. This permission he obtained, without having to resign his benefices, though he did not depart at once but was still in London in 1551. He must, however, have left soon after and settled in Urbino, his birth place, and probably from it he sent what is believed to have been his last literary composition in the form of a congratulatory letter to Queen MARY dated 5th August 1553, on her accession to the throne of England.

His death took place according to the best authority, probably at Urbino in 1555 (not 1562), for in that year various offices are recorded as being filled up *propter mortem P. Vergilii*.

THE BOOK AND THE WORK.

While the name of POLYDORE VERGIL has been held in remembrance as the author of the first attempt at a critical history of England, his other works have passed into oblivion. This

is not to be wondered at, for the themes of them have ceased to have any significance for readers, scholars or historians of the day. We care nothing for a Commentary on MARTIAL, or a treatise on Portents, or even on Proverbs, and yet they are not without some attraction when one turns them over. They were all scholarly works and showed capacity on the part of their author.

Amongst them, however, there is one, the History of Inventions, which deserves a better fate than to be utterly forgotten. It has many claims on one's attention. It was the first of its kind, and if one may judge by the editions it went through in face of all competition, it was the best. It was never superseded by anything better but came to its end only by the hand of Time. It ceased to supply the want it created and met for about 200 years, because the want itself had ceased.

THE BOOK

At this point it may not be inappropriate if a word or two be said about the book taken from the account of it I compiled some time ago (8).

The first edition of all was printed at Venice by CHRISTOFORUS DE PENSIS in 1499. Some have said that it was printed in 1498, but that is due to a misreading of the date, and it can be shown from VERGIL's own statements that the book could not have come out in that year. It is quite unnecessary, however, to discuss this question.

The early editions down to 1521—amounting to 14 or 15—were in small 4° like the first, and contained Books I-III only. The 1521 edition was in folio and in it were included not only the remaining five Books, but the Proverbs as well. Thereafter many if not most of the editions contained one or more of VERGIL's smaller tracts, and in some cases minor works on inventions by other writers like SARDUS and SABELLICUS. There were a few expurgated editions, but they had little or no circulation. The first French version and three German editions (MARTIN BRESLAUER's Cat. 10. VIII. 08) contain illustrations. The book was

(8) Bibliographical *Notes on the English Translation of Polydore Vergil's « De Inventoribus Rerum »*, 1888. Society of Antiquaries in Archeaologia.

originally written in Latin and the greater number of editions are in that language, but there are also translations in several modern languages. The number of editions is 110, or say, one hundred, making allowance for possible errors, and amongst them there are editions of every degree of rarity and abundance.

The majority of editions appeared in the 16th century, when they came in great numbers. In the 17th century the demand began to fall off and in the 18th century comparatively few appeared. The very last in the 18th century was the Dutch translation. The last of all was a reprint of the English abridgement which, in 1868, was issued privately in New York for a book club.

The first edition is a scarce book, but of certain editions only single copies are known, and these are now scattered in different libraries. If one may judge by the frequency of its appearance in catalogues, the most abundant of all the editions is the Elzevir of 1671, at Amsterdam. It is in the usual form and type of the Elzevir books. (Even it is growing scarce.)

A not uninteresting matter to consider is where the different editions appeared.

The first was published at Venice and others appeared at Rome, Florence and Brescia.

In France : 14 editions appeared at Paris and 12 at Lyons.

In Germany : 17 at Strasburg, 18 at Basel, and others at Köln, Augsburg, Magdeburg and Frankfurt.

In England : 9 or in reality 6 editions of the abridgement in English.

And in Holland : Antwerp, Ghent, Leyden, Amsterdam, Nimwegen.

Switzerland : Geneva.

Spain : Medina.

America : New York, editions were issued.

For 50 years, 1521-1575, Basel enjoyed almost a monopoly of the book, and the only other places which could compete were Paris and Lyons, and amongst them very nearly an edition a year appeared. The demand for the book must have been enormous. It is curious that not a single Latin edition appeared in London, and the translation did not appear till the book had been in circulation for 47 years. It was, one should have thought, hardly worth while to translate a book of such popularity as

this. Even when it did come out it was a mere abstract, little more indeed than what a well constructed index might have been, but the translator was afraid of what might be said or done, and he omitted much controversial matter when it was precisely that which interested the readers then. He excuses himself ingeniously for having reduced the book to the size he has done.

When one turns to the translations, one finds that out of the total number of editions, 33 are translations.

The first of all was not into Italian, the native language of the author, or English, his adopted language, but into French. It was published in 1521 and the translator was GUILLAUME MICHEL DICT DE TOURS. It is in small folio, in a peculiar Gothic letter and is extremely rare. It was reprinted in 1544. Another translation by FRANÇOIS DE BELLEFOREST appeared in 1582 and again a few years later.

The German translation by MARCUS TATIUS ALPINUS was printed in 1537 by HEINRICH STEYNER at Augsburg in folio with numerous woodcuts. This is a very rare book in folio. It was re-issued in 1544—there was another edition in 8vo without date.

There are two Italian translations—the first by PIETRO LAURO was printed at Venice by GABRIEL GIOLITO in 1543. The other was an expurgated version by BALDELLI (9) and was printed by the GIUNTI in 1587, and reissued in 1592 and 1680.

The Spanish translation was also expurgated.

The Dutch translation came late in the 18th century and is much abridged.

The English version or rather abridgement was by THOMAS LANGLEY. There are three issues of it dated 1546, one 1551, one without date about 1570. There came after a century's interval, editions in 1659, 1663, and then after 200 years' interval the American reprint already alluded to. In 1686 the matter was rearranged in alphabetical order and was issued without POLYDORE VERGIL's name.

After the book was finished and when it was found that it contained critical remarks bearing on ecclesiastical observances and other matters, it was put in the Index and a corrected edition

(9) FRANCESCO BALDELLI, printed by FILIPPO et JACOPO GIUNTI, e FRATELLI, in Fiorenza.

free from those errors was issued at Rome in 1576, and all others were condemned. But that was of no use, for the complete book continued its course, whereas the corrected edition had no circulation at all, and in 1585 the remainder was issued with a new title and preface as if it were a new book.

There is just one other point connected with the book. The first edition is dedicated to LUDOVICUS ODAXIUS, and this contains some points of biographical interest. In 1517 the author seems to have completed the last five books, and to them he prefixed a long letter to his brother MATTHAEUS, containing more biographical information. The five books and the letter were published first with the three books in 1521-1525, and then at Paris by STEPHANUS in 1528-29. Thereafter the letter was cancelled and in almost all other editions a quite different letter, also dated 1517, addressed to his brother was prefixed to the fourth book. The explanation of this change, or of the motive which led the author to make the change is not forthcoming.

As to the text Vergil rather hurried the first edition. He was afraid that his work might be forestalled by another writer —remembering what had happened to his book on Proverbs— but after he had secured the priority of publication, he revised the work in several editions, and in 1521 added the remaining 5 books, at which he must have worked during the interval. He still made additions and corrections, for the edition of 1532 shows signs of revision, but after this one, or at least after the edition of 1544, he made no further alteration. All this shows, however, that the history of inventions was a favourite work of VERGIL's and that he had it constantly before him for five and forty years.

What then was the work upon which he expended so much attention?

He calls it a work on the *Inventors of things*, and in it he tries to assign the inventions to their respective authors.

THE WORK

The treatise falls sharply into two parts. The first part is general in its scope and contains the matter which concerns the history of *Science*. The second division, books IV-VIII inclusive, deals with the origin and progress of the rites and

ceremonies of the Christian Church, and contains little or nothing bearing on invention in the strict sense of that term, though some matters, such as festivals, burials, marriage ceremonies, etc., etc., belong to that section of antiquities falling under either anthropology or folk-lore, or both. But while these ceremonials must have been started by someone, they are hardly to be reckoned discoveries, and they are not material inventions.

The course which the author pursues in the first three books —which is all with which we need be concerned at present—is moderately systematic.

He begins not with the Creation, but curiously enough with the demons or fallen angels who seduced mankind to their worship by the promise of gifts and who bulk largely as the first teachers of alchemy or transmutation to the daughters of men of all people. This chapter almost affords foundation for what has been stated that there was something of the Pagan in the author.

It would be quite beyond my power, as it would be beyond your patience, to attempt to give a review of even the first three books of the treatise. It is already so condensed by the author that a further abstract would leave very little, as is proved by the English translation. (The first half of the English translation would represent what I should have to inflict upon you, and it would be as much as you could tolerate if I were to read merely the titles of the chapters.) But the following heads of the chapters may suffice to exhibit the scope of Vergil's survey and what it involved. He enumerates the persons to whom the various discoveries have been ascribed, and gives the authorities for his statements. He has amassed therefore an immense amount of matter, from all sources, classical and medieval, and has thereby tried to make his work as exhaustive as possible. Nor is it devoid of criticism, for although he had not the facts requisite for controverting a particular statement, he was not ignorant that much of his record was drawn from sources not always trustworthy. A great deal of what is contained in the first division of the book is of a legendary character, and it is in that respect inferior to the second division where the author was on much more familiar and surer ground when he was dealing with the history and antiquities and ceremonies of the church, the rise of monastic orders and so on. Here too his criticism is more vigorous,

markedly so for the early part of the 16th century. Still it did not interfere with the circulation of the book. It would seem indeed as if his rationalizing criticism rather suited the tempers of many, however heterodox it may have seemed to ecclesiastical authority. He was evidently actuated by similar feelings and ideas to those of a very notable contemporary, CORNELIUS AGRIPPA (10), but he had them more under control, or perhaps felt less strenuously than did the famous author of the book *De Vanitate Scientiarum*. It is very striking how these two books ran parallel with one another for a couple of centuries, supplementing, and no doubt helping the circulation of each other. There is a curious analogy between the two, and each ought to be read in the light of the other. Both are histories: but Vergil's is a chronicle, Agrippa's is a sweeping invective, with history as a text.

CONTENTS OF THE BOOKS

BOOK I

1. The Origin of the Gods.
2. The beginning of things.
3. Origin of Man, languages and division of the earth.
4. Marriage-divorce ceremonies.
5. Origin of religion—adoration sacrifices.
6. Inventor of letters and who brought them to Italy, variety, etc.
7. Origin of Grammar.
8. Origin of poetry.
9. Origin of Metres.
10. Commencement of Tragedy.
11. Origin of satire and the new comedy.
12. Origin of history—invention of prose.
13. Origin of rhetoric.
14. Inventor of music.
15. Variety of instruments. Organ, flute.

(10) See Professor FERGUSON's *Bibliographical Notes on the Treatises « De occulta Philosophia and De Incertitudine et Vanitate Scientiarum » of Cornelius Agrippa*. Reprinted from the publications of the Edinburgh Bibliographical Society, 1924, Vol. XII.

16. Origin of philosophy—Ethics. Dialectic, Disputation.
17. Astrology—Course of the stars. The sphere. Navigation.
18. Geometry and arithmetic.
19. Weights and measures—and the year.
20. Medicine.
21. Discoveries of medicinal plants—composition of medicaments, honey in drugs, remedies from beasts and birds.
22. Magic—exorcism of devils—charms for disease.
23. Different parts of magic—necromancy—pyromancy—aeromancy, hydromancy, geomancy, chiromancy.
24. Two kinds of divinations—Auspices—lots—dreams.

Book II

1. Origin of Law and Laws. First Legislators.
2. Police of cities—kingship, tyranny, the crown, slavery—Ariopagus—voting and elections.
3. Triple government of Rome—Royal arms, wood of magistrates staff—numbering of the people, Senators and Knights—prisons—Lustrum Taxes.
4. Invention of the year—months. Nones, Ides, Calends—Golden number.
5. Hours—clocks.
6. Day and night.
7. Writing and publishing of books—first library—art of printing and when it was invented.
8. Use of writing. Paper. Parchment.
9. Art of memory.
10. Military art—armies—sentinels.
11. Arms—artillery.
12. Breaking of horses. Chariots—combats on horseback.
13. Olympic games and other games.
14. Games of the Romans Lupercalia.
15. Treaties, Alliances, Confederations.
16. Triumphs. Trophies.
17. Crowns—use at banquets.
18. Use of ointments.
19. Gold, silver, iron, tin, lead—tools—fire from stones—wood—lamps.

20. Money. Engraving on silver. Mirrors of silver.
21. Rings—precious stones—third finger of the left hand.
22. Glass, amber—pomatum—myrrh—crystal.
23. Images—statues.
24. Painting—colours—pencil.
25. Pottery.

Book III

1. Invention of agriculture.
2. Grinding—baking—manure—rural implements.
3. First planting of the vine and other trees—use of wine—avoidance of drunkenness—first taverners—invention of olive oil, honey, drink from barley, thickening of milk.
4. Introduction of foreign trees to Italy.
5. Who first named animals ... and who made at Rome the first parks for beasts and who first used beasts as symbols in War.
6. Invention of linen—spinning, weaving, soap, dyeing, cloth, dress, skins, carpets...
7. Invention of architecture.
8. Bricks, quarries—columns—use of marble in Rome.
9. First builders of town walls, towers—chapels—pits—ditches.
10. First labyrinths, pyramids, mausoleum, Burials—deification—funeral orations.
11. Obelisks—hieroglyphics.
12. Asylums—places of liberty.
13. Theatres—comedies—who first built them at Rome. Use of baths and stoves.
14. First tools of wood appertaining to carpentry.
15. First Commander at sea—navigation—ship building—sails, anchors, battles.
16. Merchandise—first merchants.
17. Fornication—Bacchanals. Punishment of adultery. Dyeing the hair—barbers.
18. A great many things among the ancients, the inventors of which are not known.

Book IV

1. Beginning of the Church—and of Christianity.
2. Origin of the Christian religion and its rapid progress. Martyrdom of St. Peter and St. Paul.
3. Circumcision and the trouble it gave rise to.
4. First use of baptism—not to be repeated.
5. Origin of the priesthood among the Hebrews. The chief priest—Levites.
6. Origin of the priesthood in the Christian church. Laying on of hands.
7. Ceremonies in promoting to the priesthood.
8. Origin of the custom of shaving the head. The Nazarenes—Priests debarred from second marriages.
9. Assignation of parishes to priests, and dioceses to bishops. Cardinals.
10. Honours paid to the Pope.
11. First notaries at Rome to write the lives of the Martyrs—origin of Protonotaries and Cubiculars of the pope.
12. Whence there are so many orders among the clergy—Archiepiscopal mantle. Resident canons in England—old manner of taking an oath and excommunication of rebels.
13. Sanctification of virgins—uncovering to great people—kissing the hands and feet of the pope—washing of feet.
14. Flamens. Vestals—sovereign Pontiff. Pater patratus—Augurs etc. etc. Books of the Sibyls. Bona Dea—Flora.

Book V

1. Why on festivals churches are adorned, and wax images are presented—and when a priest celebrates his first mass there is a merry banquet.
2. Distribution of largesse. Dances—theatrical plays—Mayday—torches in March. Masks at Christmas. (Praise of the English.)
3. Anointing of priests and kings—and those baptized or in the agonies of death.
4. Marriage of priests among the Hebrews and when it was stopped in the Christian church.

5. Consanguinity—several wives—power of the father—churching of women.
6. Foundation of churches—altars—cemeteries—The Cross.
7. Sacrifice among the Hebrews—festivals—dedication of the Temple. Mystery of fire.
8. Human sacrifices to devils—« Sacred spring »—end of the Oracles. Holy water.
9. Praying to the East. Preaching. The Eucharist.
10. Who first celebrated the communion. Mass. Kiss of peace. Benediction.
11. Why the celebrant says—Ite missa est. Origin of the word and the ceremonies.
12. Why in the mass are so many Hebrew, Syriac and Greek words, and their meaning.

Book VI

1. Institution of penitence. Confession—triple purgation of Bishops.
2. Inventor of canonical hours—music, its misuse in worship—heart of the temple. Writers of Hagiography.
3. Fasting—alms—advent and Lent Ember week.
4. Change of night watches to fasts—condemnation of night sacrifices.
5. Change of the names of the days.
6. Abstinence—grace before and after. Reading of scripture during meals.
7. Origin of festivals—Paschal feast—consecration of candles.
8. Penticost. Canonization.
9. Festival of the dead—mourning—white mourning by French royal widows.
10. Novemdiales Septemdial. Funerals—form of graves etc.
11. Making vows—pilgrimages with bare feet—Litany. Blessing of people who sneeze—crossing the mouth when yawning—processions.
12. Sacerdotal ornaments and vases—first ordinance of consecrating and blessing—use of bells, vails, chandeliers—banners.
13. Images among Christians—abuse of these.
14. Origin of tithes. First fruits.

Book VII

1. Beginning of the monastic life.
2. The three vows. Diversity of dress.
3. The Hieronymites, Canons regular, Augustinians, Carthusians, Carmelites, Premonstratensians, Cruciferi.
4. Cordeliers—Trinity brothers—Servants of our Lady—monks of Saint Bridget—Jesuits, New Hermits—Good Men.
5. The Holy Gens d'armes—origin and end of the Whites (Templars) Teutonics.
6. The Ninivites.
7. Sect of the priests of the Goddess *Syrie*—of Assyrians, Antonians and Ceretans.
8. Mohamed and his sect, laws and ordinances.

Book VIII

1. Honour to the relics of saints—institution of stations; Jubilee at Rome—indulgences.
2. Origin of the Pope's titles—colleges and scribes at his court—seals or bulls of lead and the use of *Annates*.
3. Birth and source of Symoniacals.
4. Origin of schisms and heresies—Luther—punishment of heretics.
5. First Councils—and whence came decrees and ordinances.
6. First persecutors of Christians—martyrs.
7. Excellence of the religion—and the state of Christians.

Added to late editions :

Bk. IX 1. Buttons and other garments, hats.
2. The Bezoar stone.
3. Alchymy. Cupel—aqua fortis.
4. Distillation.

Criticisms

All Polydore Vergil's work as well as himself was subjected by his contemporaries to unlimited criticism. Whether they were qualified either by superior, or even equal knowledge, to hold,

much less express an opinion about his work, is open to question. Whether they were or were not sufficiently unbiassed to give a rational criticism need not be questioned at all. I do not think they were competent on either point—they were not qualified and they were not unbiassed.

Having broken new ground in every subject he touched, having shown the existence of what others had overlooked, his originality was contested more or less, and where that could not be done effectively his merits were depreciated. It is unnecessary, and for my part, I shall not discuss just now at all, the question of priority as between him and ERASMUS, relatively to their collections of Proverbs. They were probably simultaneously worked out, but VERGIL's Collection was published first. There may have been a misunderstanding between the authors, but there could never have been a quarrel, and in fact they were very good friends always after the matter had been explained. As for the history of England, it was unlikely to escape criticism for several reasons. It was written by one who was not a native; it was written after a minute inspection of original authorities, and with an attempt at least at a critical estimate of them. The method was new, and the criticism made inroads upon time honoured legends and beliefs, sweeping many of them away and letting light in upon the realities of the history. This was not welcome; it was against national and race prejudices and the history was disliked and depreciated accordingly. As a foreigner it was asserted that he did not know English. Considering the number of years he lived in this country, his general ability and connection with public affairs in which he must have necessarily come in contact with persons of all ranks and kinds, it would be a very strange thing indeed if he were ignorant of the language of the country of which he was a naturalized subject. Here again, however, I leave these disputed questions alone. They have been discussed by others very fully, and as I have nothing to add to the matter, I pass it by. Moreover it does not affect the present question.

Fortunately, as concerns the History of Discoverers, none of these difficulties arise. There is no question of priority. Here VERGIL is really the first in the field, after PLINY and the medieval writers, such as ISIDORUS. In his letter to ODAXIUS he makes that

claim with a certain amount of exultation, and he is fairly entitled to it. There was only a previous writer who had made a brief catalogue of inventors a century or more prior to VERGIL. I refer to GULIELMUS PASTREGICUS, a native of Pastrengo near Verona, the intimate friend of PETRARCA who wrote a little volume on the lives of men distinguished in various directions, one of the divisions containing a list of discoverers with the inventions with which they were credited. But I doubt very much whether POLYDORE VERGIL ever saw this work, if he even heard of it. It existed only in MS. in his time and was not printed till 1547, and even that work has always been of the very rarest known, and in any case VERGIL's book is twenty times larger than the other.

There was nothing else, for the work of ZACHARIAS LILIUS, which deals with the seven liberal arts, cannot be compared in aim or execution with VERGIL's, and the poem of SABELLICUS, which is referred to by VERGIL and was printed along with VERGIL's three books in some editions, is an eloge and cannot be called a history, besides being short.

Of VERGIL's work TIRABOSCHI (11) has said that it displays the erudition and at the same time the credulity of the author.

This seems to me to be neither fair to VERGIL nor to do justice to TIRABOSCHI's historical insight and capacity.

Were it applied to a contemporary the accusation might be justified by proofs, but to apply the criteria of the close of the eighteeth century to the beliefs of 1499 cannot conduce to accurate appreciation. What ground had VERGIL to build upon? He had the whole of classical antiquity, its literature and the remains of its architecture and art. He had medieval literature, its architecture and art, and nothing more. What he knew of Egypt was drawn from the Ancients. The Hebraic economy he got from the Bible. There was no other antiquity known—Assyria and Babylon were mere names, they are not even mentioned; of prehistoric man, he could know nothing—there were no excavations. He was therefore at the mercy of his sources of information. He might doubt them, but he had no means of criticizing them. He had to accept what they told him or leave it alone. He knew that quite well himself, for in his letter to

(11) GEROLAMO TIRABOSCHI, 1731-1794.

ODAXIUS he admits accepting fables, but he excused himself because they contained a substratum of truth, and when inventions were ascribed to Saturn or Neptune or Mercury, he took them as names of mortal inventors rather than as gods. In short VERGIL did the best he could with his material which he had gathered by very great research, as appears from his authorities, although the actual composition of the book, he tells us, took him about nine months. He was unable, literally unable, to question his authorities. He had no reason to do so—except the inherent improbability of what they advanced, and if he had tried, he must have failed from lack of the requisite knowledge.

VERGIL affords but another instance of the overwhelming fact that no one can transcend his own time. The work he does is up to the limit of the knowledge then, and if he is able to advance a little way beyond, his new position serves but as a vantage ground for some one else to go farther still.

That the History, an outstanding work of the end of the 15th and beginning of the 16th century, should have been still in active circulation down to the beginning of the 18th, shows that in spite of the many advances made in geographical discovery and in science during all that time, no one was found to revise the book, modernize it and incorporate the discoveries and inventions that had been made. If that had been done, if VERGIL had taken example by his contemporaries, GREGORIUS REISCH (12) and AUGUSTINUS JUSTINIANUS (13), and included the new world, the mariners compass, and the other novelties of the early 16th century, and thus set an example of revision, his book might almost have continued down to the present time. But as far as the general subject was concerned, men were standing still. And so VERGIL's book was reprinted without alteration until the time came when the changes and advances that had been going on silently put an end not merely to the interest in the questions VERGIL had discussed, but actually to the questions themselves and to the mode of discussing them. VERGIL's authorities had ceased to have any weight : the experimental method

(12) See Professor FERGUSON's *Bibliography of the Margarita Philosophica of Gregorius Reisch*, published by the London Bibliographical Society, 1929.

(13) AGOSTINO GIUSTINIANI, born at Genoa in 1470. Editor of Polyglot Bible, 1516, published at Genoa. Died in 1536.

of testing facts and phenomena had arisen and the whole epoch came to an end. We look back on it now as a phase of European thought and deal with it historically.

But all that does not affect VERGIL's merit or his position and the estimate that we at this time can form of him.

CONCLUSION

If any one ever deserved to be included in a history of inventors for the discovery or invention of a new subject and for working it out, it is POLYDORE VERGIL; and if any one ever deserved recognition at the hands of a section such as this, devoted to the history of discovery and invention, it is surely the man who pointed out that there is such a topic at all as the History of Inventions. He has besides international claims, for he belonged to two nations and his work was not only written for scholars in what was once the international language of scholars, but it was translated into the vernacular of half a dozen countries, into Dutch, English, French, German, Italian and Spanish. He might very well therefore be put up as the progenitor of this section—the Urgrossvater—for he was working on the subject and collecting material for it in 1498, which is just 410 years ago.

Science in Early English Literature

1550 to 1650

An Attempt to Trace the Scientific Opinions of the People of England as Reflected in their Books, from the Time Scientific Works were First Published in the English Language to the Date of the Establishment of the Royal Society.

TO THE READER

It is fair to reiterate that the present survey is but an attempt to present a large scene on a small canvas. No searching through this early literature could be uninteresting or unfruitful; the difficulty is, rather, to restrict one's interest to the topic in hand, and to keep from wandering in many an alluring by-path. Persons with a wider knowledge of science and literature could doubtless suggest many gaps in the record. and even draw from the gleanings here recorded, weightier and more valuable conclusions. Yet it is believed the selections made are representative of the period, and the deductions drawn therefrom reasonable and fair. It is not all of it easy reading. Indeed « a work of this nature is not to be performed upon one legg; and should smel of oyl, if duly and deservedly handled ». Even so is it hoped that the interested reader will at least find some useful hints towards a further investigation, on which it is acknowledged many kilowatt hours might profitably be spent.

K. M.

Contents. I. The social and intellectual background; II. The science of numbers; III. The new astronomy; IV. Astrology; V. Alchemy; VI. Maps and maritime matters; VII. Science in war and in peace; Bibliography.

CHAPTER I

THE SOCIAL AND INTELLECTUAL BACKGROUND

> « We hope it will not be unconsidered, that we find no open tract, or constant manudiction in this Labyrinth; but are oft-times fain to wander in the America and untravelled parts of Truth. »
>
> Sir THOMAS BROWNE (1).

That literature reflects life is a theory often presented, and very widely accepted. There are even those who claim for the imaginative record precedence over formal history as a true picture of a given period. And if true, this picture must include to a greater or less degree, according to the character of the era, an outline of the progress of science, with the effect upon everyday living and thinking of the new discoveries and inventions. For, if the poet is the best of all historians, it is because in reacting to events the creative imagination is moved not merely to reflect, but to illuminate, the outward happening, and to interpret its significance. In the present highly mechanical age, for example, the real spirit of the times may perhaps be found not in histories of invention or essays on science, but in such stirring dramas as *R.U.R.*, *Dynamo*, and *Wings over Europe*, with their note of warning and of dread.

In such an expansive period as the sixteenth and early seventeenth centuries in England, while literature and science may appear to have pursued their separate ways with little mutual concourse, the same influences were steadily operating, and it should be possible to trace them. Already known as a « merchant island », with a rising middle class, more readers, and a general eagerness for change and adventure, England was prepared to reward the invention of anything that contributed to her prestige or prosperity. The stimulus to scientific investigation therefore lay in the direction of improved implements of war, aids to navigation, and better means of communication.

With this spur behind the enlarged imagination of the time, it is inevitable that the rich outburst of literary effort in the

(1) *Pseudodoxia epidemica*, 1646. Preface. (Works, ed. by WILKIN, v. 1, p. 3.)

Elizabethan age should be paralleled by a widespread scientific awakening. Much of this, it is true, found expression in vague and fantastic theories, and until the publication of GILBERT's *De Magnete* in 1600 there was little attempt at correlation of such discoveries as were made. But the awakening was there, and the gropings towards a systematic philosophy quite appropriately came to a focus in his great contribution to the knowledge of magnetism, with its immediate application in the field of navigation.

Although printing had been introduced into England in 1475, the spread of reading was largely through the acquisition of books printed on the continent, and this was especially true of scientific works. CHAUCER's *Treatise on the Astrolabe* (2), addressed to his little son, is thought to be the first English work on a scientific subject, and he finds it necessary to apologize for writing in the vernacular, calling on the boy to « pray God save the king that is lord of this language » (3). It was not, in fact, until the middle of the sixteenth century that scientific books began to be generally written in English, and Sir THOMAS BROWNE opines : « If elegancy still proceedeth, and English Pens maintain that stream we have of late observed to flow from many, we shall, within few years, be fain to learn Latine to understand English » (4).

Few references to scientific phenomena are therefore to be looked for in the earlier English literature. In letters, chronicles, and miscellanies, however, we find evidence that Englishmen were following continental investigations with keen interest. By the beginning of the seventeenth century this had grown to a *mutual* intercourse with foreign scientists, and GALILEO is calling GILBERT *il grandissimo filosofo* and declaring that « his marvelous conception (that of the earth being a great loadstone) was to him a subject of praise, admiration, and envy » (5).

The profession of literature was evidently recognized as one to which a man might devote himself wholly, but except for the mathematicians most of the scientists of the time were indulging

(2) Written in 1387; reprinted in *Early science in Oxford*, v. 5, 1929.

(3) *Ibid.*, p. 2.

(4) *Pseudodoxia epidemica*, 1646. Preface. (Works, ed. by WILKIN, v. 1, p. 3.)

(5) *Private life of Galileo, compiled principally from his correspondence...* London, 1870. pp. 35-36, footnotes.

their taste for learning as a side-line or personal hobby. Thus, GILBERT, MARK RIDLEY, and Sir THOMAS BROWNE were physicians; WILLIAM BARLOWE and GEORGE HAKEWILL, Archdeacons; BACON and WILLIAM BOROUGH high in affairs of state and government. Apparently a certain rivalry existed as to the worth of these various pursuits, for we find Sir PHILIP SIDNEY defending the poet's calling against two charges of the scientists: first, that a man might better spend his time, which he denied, since no learning is so good as that which moveth to virtue, and « none can both teach and move thereto so much as Poetry » (6); and second, that poets were liars, to which he answered stoutly « that of all Writers under the sunne, the Poet is the least liar; and though he would, as a Poet can scarcely be a lyer, the Astronomer, with his cosen the Geometrician, can hardly escape, when they take upon them to measure the height of the starres... Now, for the Poet, he nothing affirmes, and therefore never lyeth... and therefore, though he recount things not true, yet because he telleth them not for true, he lyeth not » (7).

The very name of scientist was still to be coined (8), laborers in this field of knowledge being known, rather, as Artificers, Schollers, Mechanicians, Practitioners, Experimenters, Secretaries of Arte and of Nature,—occasionally even as Ingeniors (Inginers, Enginers), Mathematicians, and Astronomers—but above all and most generally, as *Philosophers*. In JOHN EARLE's *Micro-cosmographie* of 1628, analyzing fifty-four « characters », no scientist is included. The nearest in type is perhaps the « downe-right Scholler », who « cannot speake to a Dogge in his own Dialect, and understands Greeke better than the language of a Falconer. Hee has beene used to a darke roome, and darke Clothes, and his eyes dazzle at a Sattin Doublet » (9).

Yet the record is not lacking that these men of kindred taste, call them what we will, were wont to foregather for mutual encouragement. « Divers worthy persons inquisitive into natural

(6) *The defense of poesy*, 1595. (Harvard classics, v. 27, p. 35.)

(7) *Ibid.*, p. 36.

(8) « We need very much a name to describe a cultivator of science in general. I should incline to call him a Scientist. » WHEWELL, *Philosophy of the inductive sciences*. London, 1840. Aphorism XVI, 4. v. 1, p. CXIII.

(9) Character no. 20. (English reprints, ed. by ARBER, p. 41.)

philosophy » are said to have met at times at the house of WILLIAM GILBERT, and lectures on mathematics and navigation were given in London even before the organization of the « Invisible College » of ROBERT BOYLE. Out of these beginnings came, in due time, the Royal Society, and with the Restoration a new era opened in science and literature as well as in social life and government.

Setting aside the formal records, then, which at best are meagre, let us see what the literature of the hundred years preceding the formation of the Royal Society has to tell us of the earlier progress of science in England.

CHAPTER II

THE SCIENCE OF NUMBERS

> « He therefore that shall contempte number, he declareth himselfe as brutishe as a beaste, and unworthy to be counted in the felowshippe of men. »
>
> RALPH RECORDE (10).

Mathematics, « the whetstone of witte » (11), was cultivated for its own sake as a pastime and an intellectual diversion, but not yet linked to any extent with experiment as a means of solving physical problems. The natural zest of the mathematicians was further stimulated by the practice of publicly proposing problems for solution as a sort of challenge from individual scientists, one to another. There was much correspondence on the subject, and an eager exchange of results and opinions (12).

WILLIAM LOWER writes that « The three vexations of scientificall mortals hath held me most, to wit, the squaringe of the circle, the dublinge of the cube, and the philosopher's stoone » (13). In the spring of 1611 he addresses several letters to THOMAS HARRIOT in regard to the latter's « dogs » :

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(10) *The grounde of artes.* London, 1542. Preface.

(11) *The whetstone of witte, whiche is the seconde parte of arithmetike,* was published by RECORDE in 1557.

(12) *See* HALLIWELL-PHILLIPPS, J. O. *A collection of letters illustrative of the progress of science in England from the reign of Queen Elizabeth to that of Charles the Second.* London, 1841.

(13) *Ibid.*, p. 42.

I never loved huntinge till you furnished mee with dogs, I will henceforward prove another Nimrode; indeed I sought both arkes and tangents in the tables of proportion onlie; ... only your curre dog Petiscus will not come into my sighte; but after a weekes vaine anger and points in seeking for him, I found at last it was not the poore curres fault, but Mr. Bills, that, sendinge me the second edition of the *Origonometria*, sente me onlie the tables; so... I found no such thing as you sited, nor no such examples as you added... So you see I am defeated of the curre dog, savinge what I pick out of your letter, which indeed is not enough to give me satisfaction. Let Kitt, when he goes next to London, speake to Bill... for a perfect copie of Petiscus... Your dog, that hunts by the sines onlie, and I am growen familiar, and he is an excellent dog; but your she bitch hath no fellow for... onlie she is slow; so that by the assistance of thes dogs of yours I grow so confident as to undertake to pursue in chace anie game : but then onlie I shal be sure that nothing doe escape me, when you shall please to importe unto me a betch of your triangular kindes...

By the helpe of your dogs, I will revew all thos workes of anie that exhibite the distance in the ellipses, equal or neerest to the same distance found before... and when I have perfected them, I will send them unto you, with all the numbers given and found (14).

Comparing the famous VIETA's solution of a problem with HARRIOT's method, he confesses :—« I can have my will of neither », though « I proceed still with much pleasure and satisfaction ». This letter concludes with the rueful declaration : « all well except my catle, which have al this winter been persecuted with the murraine; since Christmas verie neere I have lost 100 beastes— Vieta's sacrifices to the witch Melutina for the invention of one probleme » (15).

Another famous mathematician, HENRY BRIGGS, writes from Merton College : « I am still at my logarithms, and can neither finish them to my mind nor let them alone ». He entreats THOMAS LYDYAT to « get out your meditations... and demonstrate everything as you go » (16). But LYDYAT has other matters on his mind and replies that he will supply demonstrations only when necessary. Referring to three « inchoate and imperfect treatises on astronomy », he reveals the contemporary viewpoint on another scientific project by declaring : « But speed these as they may, with diagrammes or without, I am resolved against the bringing in of the Gregorian year and calendar into our country, to oppose my great Period or Annus Magnus; and, with God's

(14) *Ibid.*, pp. 38-39.
(15) *Ibid.*, p. 41.
(16) *Ibid.*, p. 46.

help, to maintain against whatsoever Jesuit or Papist... (17) »

Leisure to pursue his speculations was afforded Mr. LYDYAT, as well as many another needy investigator of the period, by a long stay in the debtors' prison. Writing again to HENRY BRIGGS, he advocates the taking of astronomical observations « under the aequinoctial, and beyond the farther tropic, to be compared with ours », offering himself to go either under the North pole or over the Cape of Good Hope, « to the one place or to the other, even to the world's end... so out of prison » (18). He proposes, in the fashion of the day, the organization of an expedition under a noble patron « to give yet another essay towards the finding of the Northern passage to the west of America and the East Indies, so long sought for by our English nation ». As a preliminary he asks for thirteen hundred pounds as a loan for payment of his debts, and a pair of the newest and best globes and twenty nobles in money to buy books. « But in the meantime I want fourty marks to print mine almanacks : and (I should have said first) to buy me a suit of apparel to defend me from the cold » (19).

Following his scientific interests wherever they led him, Sir CHARLES CAVENDISH traveled to the homes of various continental authors of repute. In his vigorous correspondence with JOHN PELL reporting his experiences may be traced a running bibliography of European science :

I have not (to my remembrance) seene *Henischius arithmetick*, nor should desire, for his mislike of demonstration by letters (20).

Here I mett with *Sir Kenelm Digbie's booke*, but had no time to reade it all, but it appeares to me to have some things in it extraordinarie (21).

Wee met here with the famous Cappuchin *Rieta, his booke* is now in the press heere almost finished. Hee solves the aparences of the planetes by excentricks, without aequants or epicycles, and yet not according to Copernicus... He teaches allso in this boke the making of his newe tellescope... doubtless he is an excellent man and verie courteous (22).

I am extremely taken with *Des Cartes his new booke*, yet I think *Kercher the*

(17) *Ibid.*, p. 48. The Gregorian calendar, devised in 1582, was not adopted in England until 1752.

(18) *Ibid.*, p. 58.

(19) *Ibid.*, pp. 54-55. Letter to Sir HENRY MARTIN, written October 17 to November 30, 1626.

(20) *Ibid.*, p. 74.

(21) *Ibid.*, p. 77.

(22) *Ibid.*, pp. 77-78.

jesuit of the loadstone hath prevented Des Cartes, for they differ little as I remember (23).

Mr. Hobbes writes *Gassendus his philosophie* is not yet printed, but that he hath read it, and that it is as big as Aristotle's philosophie, but much truer and excellent Latin (24).

Though you discommend *Niceron* for a vainglorious man, yet your naming of his booke commended it to me (25).

JOHN PELL was at that time (the 1640's) the leading mathematician of England. In fact, Sir CHARLES, although inquiring about the « analyticall » and « analogicall worcke » of other experimenters, says : « I confess I expect not an exact booke of analiticks till you perfect yours » (26); and Sir WILLIAM PETTY writes from Paris : « Those rules of algebra (though few) which you gave mee... have made mee able to doe many pretty questions. I entend to reade no autour of that subject untill I may be so happie to reade something of yours » (27). Among PELL's claims to contemporary fame was his refutation by two methods of the alleged quadrature of the circle, one refuting those that make the circle's area too great, and another for those who make it too little. A copy of this refutation was sent to LONGOMONTANUS, the aged claimant of the solution, who « being strangely enamored of his invention could not bee made to retract it; and hee hath grown extreme old in his dotage thereon ». « Wherefore », says GOLIUS, « 'twere scarce religion to trouble the obstinat old man any more, since other thoughts would better become his yeares than the mathematicks » (28).

Twenty years later PELL's approval was sought for an alleged solution of another great mathematical problem :

This morning Mr. Thomas Hobbes met me in the Strand, and led me back to Salisbury House, where he brought me into his chamber, and there shewed me his construction of that Probleme, which he said he had solved, namely, the Doubling of a Cube. He then told me that Vicount Brounker was writing against him... but he intended to have his demonstration printed and sent to transmarine mathematicians... Whereupon he gave me these two papers, bidding me take as much time as I pleased. Well, said I, if your work seeme true to mee, I shall

(23) *Ibid.*, p. 84.
(24) *Ibid.*, p. 85.
(25) *Ibid.*, p. 85.
(26) *Ibid.*, p. 74.
(27) *Ibid.*, p. 91.
(28) *Ibid.*, p. 82.

not be afraid to tell the *world* so; but if I find it false, you will be content that I tell *you* so; but privately, seeing you have onely thus privately desired my opinion of it... (29)

There was apparently a general exchange of mathematical lore in manuscript before publication, and in the troublous days of Parliamentary strife in England many of these papers were lost. Upon the death of WALTER WARNER, for example, considerable concern was felt over the preservation of his *Analogickes*, which were ultimately found but in too incomplete a state for publication. Writing from Amsterdam, on August 7, 1644, Mr. PELL reports on the affair to Sir CHARLES CAVENDISH as follows :

« You remember that his papers were given to his kinsman, a merchant in London, who sent his partner to bury the old man : himselfe being hindered by a *politicke gout*, which made him keepe out of their sight that urged him to contribute to the parliament's assistance, from which he was exceedingly averse... In the meane time I am not a little afraid that all Mr. Warner's papers, and no small share of my labours therein, are seazed upon, and most unmathematically divided between the sequestrators and creditors, who (being not able to ballance the account where there appears so many numbers, and much troubled at the sight of so many crosses and circles in the superstitious Algebra and that black arte of Geometry) will, no doubt, determine once in their lives to become figure-casters, and so vote them all to be throwen into the fire, if some good body doe not reprieve them for pye-bottoms, for which purposes you know analogicall numbers are incomparably apt, if they be accurately calculated.

But should that befall Mr. Warner's papers and mine which we feare, it would put me into an humour quite contrary to that in which I have hitherto beene. I have thought nothing elaborate enough to be printed, till it were so complete that no man could better it, and did therefore so long keepe my name out of the presse : but now I begin to count nothing safe enough till it be printed, and therefore I have almost resolved to secure my thoughts, not by burying my papers in England... but by publishing the same notions heere that I have committed to paper there » (30).

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(29) *Ibid.*, p. 96.
(30) *Ibid.*, pp. 80-81.

CHAPTER III

THE NEW ASTRONOMY

« Science hath no enemie but the ignoraunt. »
THOMAS NASHE. (31)

The outstanding contribution of the period to speculative science was, of course, the Copernican theory, first published in 1543. This revolutionary doctrine, so apparently contrary to British common sense, and so upsetting to the theologians, Protestant as well as Catholic, cannot be said to have been welcomed in England. Being only a hypothesis, and at that time unsupported (and, in the lack of suitable instruments, unsupportable) by accurate observation, it was but gradually and somewhat cautiously accepted even by the mathematicians and astronomers.

When ROBERT RECORDE, in one of his « Master and Scholar » dialogues in the *Castle of Knowledge*, sets forth with implied approval the Copernican thesis « that the earth not only moveth circularlye about his own centre, but also may be, yea and is continually out of the precise centre 38 hundredth thousand miles », he is met with the typical reaction :

Scholar. Nay syr in good faith, I desire not to heare such vaine phantasies, so farre against common reason, and repugnante to the consente of all the learned multitude of Wryters, and therefore let it passe for ever, and a daye longer (32).

Some French verses translated in 1591 similarly assert :

« Those clerks that think—think how absurd a jest !
That neither heavens nor stars do turn at all,
Nor dance around this great round earthly ball,
But the earth itself, this massy globe of our's,
Turns round about once every twice twelve hours !
And we resemble land-bred navvies
New brought aboard to venture on the seas;
Who at first launching from the shore suppose
The ship stands still and that the firm earth goes » (33).

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(31) *The anatomie of absurditie*, 1589. (Works, ed. by MCKERROW, I.35.37.)
(32) London, 1556, p. 165.
(33) DU BARTAS. *The divine weeks*. (SYLVESTER's translation ed. by HAIGHT, Preface pp. XX-XXIII and note.)

The *Anatomy of Melancholy* ridicules those who invent new systems of the world, correcting others, doing worse themselves, « as a tinker stops one hole and makes two; ... they hoyse the earth up and down like a ball, make it stand and goe at their pleasures»(34). SHAKESPEARE seems to declare now for the earth's supremacy, and again to hint at an understanding of the central position of the sun :

> The heavens themselves, the planets *and this centre*,
> Observe degree, priority, and place...
> And therefore is the glorious planet, Sol,
> In noble eminence enthron'd and spher'd
> Amidst the other... (35)

He is aware of the prevailing controversy, but does not consider the subject profitable business for poetic contemplation :

> Study is like the heaven's glorious sun,
> That will not be deep-search'd with saucy looks;
> Small have continual plodders ever won,
> Save base authority from others' books.
> These earthly godfathers of heaven's lights,
> That give a name to every fixed star,
> Have no more profit of their shining nights
> Than those that walk and wot not what they are (36).

MILTON, wavering between the old astronomy and the new, has his Archangel reply to Adam :

> Whether the sun predominant in Heaven
> Rise on the Earth, or Earth rise on the Sun,
> .....
> Solicit not thy thoughts with matters hid,
> Leave them to God above, him serve and feare.
> . . . . . be lowlie wise (37).

First among the scientists, JOHN FIELD, in the *Ephemeris* for 1557 (38), takes his stand with COPERNICUS, and in the preface

(34) Pt. 2, Sec. 2, Mem. 3 (ed. of 1632, p. 255.)
(35) *Troilus and Cressida*, I.3.85-91.
(36) *Love's labor lost*, I.1.84-91.
(37) *Paradise lost*, VIII. (Harvard Classics v. 4, p. 250.)
(38) *Notices of English mathematical and astronomical writers...* 1837. p. 39. (Published anonymously, but attributed by HALLIWELL-PHILLIPPS to Professor DEMORGAN.)

to the same work JOHN DEE expresses a like conviction of the truth of the heliocentric theory. Another noted mathematician, THOMAS DIGGES, states his dissatisfaction with the Ptolemaic system as being « like a set of hands, head and feet taken off differend men » (39), which he logically avers could not have been the case if the hypotheses were true. Similarly dissatisfied with the old notions of the solidity of the heavens, BACON declares : « It is the absurdity of these opinions that has driven men to the diurnal motion; which I am convinced is most false » (40). He had previously conceded that while contrary to natural philosophy, it might serve as a possible explanation according to astronomy. But to the end he found the Copernican scheme « inconvenient » (41), and, like TYCHO BRAHE, preferred to invent a system of his own, which won little contemporary adherence, and is now entirely forgotten. The first really prominent man of science in England to make public proclamation of his conversion to the new astronomy was WILLIAM GILBERT in 1600 (42), and as late as 1620, we find the *Novum Organum* still asserting that the earth's movement cannot be allowed (43).

A French theorist opposed the system on the ground that « rest is nobler than movement » (44); conservative thinkers, always opposed to change, agreed with LUTHER that « the fool will overturn the whole science of astronomy » (45); and a Dean of the Church of England found « 80-odd express places in the Bible affirming in plaine and overt terms the natural and perpetuall motion of sun and moon » (46). Yet another English clergyman, the open-

(39) *Geometrical practice*. 1571. (Quoted from *Shakespeare's England*, v. 1, p. 447.)

(40) *De augmentis scientiarum*. Bk. III, Ch. 4. (Works, ed. by SPEDDING, v. 4, p. 348.)

(41) *Descriptio globi intellectualis*. Ch. 5. (Works, ed. by SPEDDING, v. 5, p. 517.)

(42) *De Magnete*. Bk. VI, ch. 3-5, pp. 214-230.

(43) Bk. II, ch. 46. (Works, ed. by SPEDDING, v. 4, p. 212.)

(44) BODIN. *Universae naturae theatrum*. 1596. (Quoted in *The gradual acceptance of the Copernican theory...* by DOROTHY STIMSON, p. 46.)

(45) « Der Narr will die ganze Kunst Astronomie umkehren. » Tischreden, IV, 575. (Sämmtliche Schriften. Leipzig, 1844-45, Bd. 22.)

(46) Marginal note by Dean WREN of Windsor (father of the famous architect) in copy of BROWNE's *Pseudodoxia epidemica*. (Quoted in *The gradual acceptance of the Copernican theory...* by DOROTHY STIMSON, p. 88.)

minded JOHN WILKINS (47), brought out (though anonymously) in 1640 a « Discourse concerning a New Planet. Tending to prove, that ('tis probable) our Earth is one of the Planets », in which he proposes the excellent rule « that men should give soft words and hard arguments » (48) in all disputes. He applies his own tenets by suggesting the following propositions in regard to the Copernican controversy (49) :

That the seeming Novelty and Singularity of this Opinion can be no sufficient Reason to prove it erroneous.

That the Places of Scripture which seem to intimate the Diurnal Motion of the Sun or Heavens, are fairly capable of another interpretation.

That there is not any Argument from the Words of Scripture, Principles of Nature, or Observations in Astronomy, which can sufficiently evidence the Earth to be in the Center of the Universe.

That it is more probable the Earth does move, than the Heavens.

That this Hypothesis is exactly agreeable to Common Appearances.

Thus, the old imagery, old philosophy, and old speculative opinion were alike slow in yielding to the claims of science, and not for a century did the new astronomy wholly win its way in England.

CHAPTER IV

ASTROLOGY

« Many affirm, and most, I perceive, deny not. »
Sir THOMAS BROWNE (50).

The faith which Elizabethans denied to true science as represented by the work of COPERNICUS was, meanwhile, abundantly lavished on the pseudo-sciences of astrology and alchemy. The universality of belief in astrology, says Professor DEMORGAN (51), is proved by observing how few works were

(47) Afterwards Warden of Wadham College, Master of Trinity College, Bishop of Chester, and the first secretary of the Royal Society.

(48) Preface.

(49) Chapter headings.

(50) *Pseudodoxia epidemica*. Bk. III, ch. 11. (Works, ed. by WILKIN, v. 2, p. 434.)

(51) See *Notices of English mathematical... writers*, p. 25, footnote.

written against it, and belief in the efficacy of hidden forces for both good and evil was the principal answer of the times to the stupendous mysteries of nature. Even among the scientists a critical attitude towards certain hypotheses was often accompanied by a ready acceptance of other utterly untested notions of cause and effect.

Thus, the same JOHN DEE, who, in advance of his time, could rise to the demands of the new astronomy, is said to have persuaded himself and others that he dealt with evil spirits. Certain it is, that he made his livelihood as a necromancer, magician and astrologer (52). Sir THOMAS BROWNE, on the contrary, held firmly that « To make a revolution every day is the nature of the sun, because of that necessary course which God hath ordained it » (53). Yet he questioned the claims of the Astrologers « which pretend to be of Cabala with the stars, (such I mean as abuse that worthy inquiry)... who, having won their belief unto principles whereof they make great doubt themselves, have made them believe that arbitrary events below have necessary causes above » (54).

Sir WALTER RALEIGH thinks « it cannot be doubted but the Stars are instruments of far greater use than to give an obscure light, and for man to gaze on after sunset » but, steering a middle course, he balances all opinions, and decides that « God... could not, contrary to his merciful nature, be so unjust as to bind us inevitably to the destinies or influences of the stars... (55) » « And therefore as I do not consent with those who would make those glorious creatures of God virtueless, so I think that we derogate from His eternal and absolute power and providence, to ascribe to them the same dominion over our immortal souls, which they have over all bodily substances and perishable matters » (56). He finally cites with approval the saying of PLOTINUS, that the stars were significant but not efficient.

In the imaginative writing of the period, references to the planets and their influence are almost innumerable.

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(52) *Ibid.*, p. 37; also *Shakespeare's England*, v. 1, pp. 456, 471-72.

(53) *Religio medici*, 1642-43. Sec. XVI. (Works, ed. by WILKIN, v. 2, p. 341.)

(54) *Pseudodoxia epidemica*. (Works, ed. by WILKIN, v. 1, p. 24.)

(55) *History of the world*, 1614. Bk. I, Sec. XI. (Oxford Press, 1729, p. 28.)

(56) *Ibid.*, p. 33.

> But when the planets, in evil mixture, to disorder wander,
> What plagues, and what portents! what mutiny!
> ... frights, changes horrors,
> Divert and crack, rend and deracinate
> The unity and married calm of states
> Quite from their fixture (57)!

SHAKESPEARE speaks also of « a charitable star », « favorable stars », « the luckiest stars in heaven », « thwarting stars », « bad revolting stars », « malignant and illboding stars ». Some of these allusions are doubtless merely metaphorical, yet the choice of the symbol shows how widespread was the current superstition. Not that the age was without its doubters, or that SHAKESPEARE failed to represent them. When Glendower boasts,

> ... at my birth
> The front of heaven was full of fiery shapes.
> ...
> I can call spirits from the vasty deep.

Hotspur retorts in thoroughly modern fashion :

> Why, so can I, or so can any man.
> But will they come, when you do call for them (58)?

And there is Caesar's wise rejoinder :

> The fault, dear Brutus, is not in our stars,
> But in ourselves, that we are underlings (59).

The very beauty and effectiveness of this celestial imagery may well have caused it to persist beyond the time when it would be taken literally. For do we not still, with Malvolio, « thank our stars », when fortune favors us?

Among the prose writers astrology is likewise a favorite topic. ROBERT GREENE treats the subject fancifully (60), THOMAS NASHE, satirically (61), while the HARVEYS apply their acute minds to

(57) *Troilus and Cressida*, I.3.94.
(58) *I Henry IV*, III.1.37-54.
(59) *Julius Caesar*, I.11.139-140.
(60) *Planetomachia*. London, 1585.
(61) *Wonderfull, strange and miraculous, astrologicall prognostication for this year of our Lord God* 1591.

a serious investigation of its claims. Since they have been blamed for sharing the credulity of their age, it is fair to record that GABRIEL tried at one time to persuade his brothers to restrict their attention to astronomy « without mixture of things to come, or discouerie of hidden and secret matters » (62). He took little account, indeed, of « any study, meditation, conference or Exercise, that importeth not effectual use » (63). And in that age of extravagant hypotheses it is something to have discerned : « As in other businesse, so in learninge... one perfect Mechanician worth ten unperfect Philosophers » (64).

This reasonable attitude is not, it is true, sustained in all the writings of GABRIEL HARVEY. He published a series of popular Almanacks with the usual prophecies and prognostications, and in his « Pleasant and Pitthy familiar Discourse » concerning earthquakes (65) he exhibits almost every shade of current opinion. There is an allusion to « the favourable and gratious aspect of some blessed planet » (66), as well as a reverent acknowledgement of « a Supernaturall Cause, to whom nothing at all is impossible » (67). The catastrophe is plausibly, if facetiously, described to *Mystresse Inquisitiva* and *Madame Incredula* as resulting from a conflict between the « sundry Accidental humours, and fumes, and spirites, either good, or bad, or mixte » within the body of the earth; and, again, as « the Earth having taken in too much drinke, and as it were over lavish Cups (as it hath sensibly done in a manner all this Winter past) now staggereth, and reeleth, and tottereth, this way and that way, up and downe, like a drunken man, or wooman »; while « a few new Contradictorie fellowes make no more of it, but a certain vehement, and passionate neesing, or sobbing, or coffing... (68) »

These simple explanations for the benefit of the Gentlewomen are followed by a more serious interpretation of the phenomenon

(62) *See* NASHE. Works, ed. by MCKERROW. Introd. v. 1, p. 67, footnote.
(63) *Foure letters and certain sonnets*. 1592. The fourth letter. (Works, ed. by GROSART, v. 1, p. 228.)
(64) *Ibid.*, p. 230.
(65) *Three proper and wittie, familiar letters lately passed betweene two Universitie men touching the earthquake in Aprill last...* London, 1580.
(66) *Ibid.* (Works, ed. by GROSART, v. 1, p. 50.)
(67) *Ibid.*, p. 54.
(68) *Ibid.*, p. 48.

for stouter masculine minds, in which a theory of natural causes is somewhat oracularly presented. Thus, the reader may believe it a testament of the wrath of God, a portent of plague and pestilence, or merely the effort of the winds imprisoned in the caves of the earth « to get them home to their Naturall lodgings. (69) » The discussion closes, however, with a plea for a serious study of the subject by « some learned and well advised Universitie man », and proffers the pertinent interrogation : « But what say you nowe, quoth I, to the staying and quieting of the Earthe, beeing once a mooving? May it not seeme a more myraculous worke, and greater wonderment, that it shoulde so suddainely staye againe, being mooved, than that it should so suddainely moove, beyng quiet and still? (70) »

GREENE's *Planetomachia* undertakes to discover through « pleasant and Tragicall histories » the essence, nature, and influence of the seven planets for the enlightenment of humanity. It is chiefly, however, a dispute between Venus and Saturn as to the relatively baleful or beneficent effect of their respective influence on human affairs. Although *Sol*, as Moderator, gives the decision in favor of Venus, the result is not consoling since all the ***dramatis personae*** of both pleasant tragedies meet with violent deaths in rapid succession.

The publication of RICHARD HARVEY's *Astrological Discourse* (71), predicting, with attendant evils, the conjunction of Saturn and Jupiter, which failed to take place, aroused a storm of ridicule. It was broadly satirized by THOMAS NASHE in his « Wonderfull strange and miraculous Astrologicall Prognostication for this year of our Lord God 1591, Discovering such wonders to happen this yeere as never chaunced since Noes floud ». The pamphlet, brought out as by « Adam Fouleweather, Student in Asse-tronomy », shows NASHE's « scoffing, satirical, and merry style » at its best, the title concluding with the challenge : « Wherein if there be found one lye, the Author will loose his credit for ever » (72).

How much this scepticism of NASHE's was due to a genuine critical faculty is difficult to determine. Since the HARVEYS

(69) *Ibid.*, p. 52.
(70) *Ibid.*, p. 60.
(71) Published in 1583.
(72) Works, ed. by MCKERROW, III.377.

believed in astrology, he would naturally ridicule it, though he asserts : « I am... a Scholler in something else but contention » (73). This claim is in truth supported by a quizzical attitude in the face of other popular beliefs of the day. In the *Terrors of the Night* he scoffs at « dappert Mounsier Diego, who lives by telling of newes, and false dice, and it may be hath a pretie insight into the cardes also, together with a little skill in his Iacobs staffe and hys compasse; being able at all times to discover a new passage to Virginia... In secret be it spoken, he is not so great with the divell as you take it. It may be they are neere a kinne, but yet you have manie kindred that will doo nothing for one another; no more will the devell for him, except it be to damne him » (74).

He reaches the sensible conclusion that « a Dreame is nothing else but the eccho of our conceipts in the day » (75), and continues :

> Just such like impostures as is this Art of exposition of dreames, are the artes of Phisiognomie and Palmestrie; wherein who beareth most palme and praise, is the palpablest foole and Crepundio. Lives there anie such slowe yce-braind beefe-witted gull, who by the riveld barke or outward rynde of a tree will take upon him to forespeak how long it shall stand, what mischance of wormes, caterpillers, boughs breaking, frost bitings, cattells rubbing against, it shall have? As absurd it is, by the external branched seames or furrowed wrinckles in a man's face or hand, in particular or generall to coniecture and foredoome of his fate (76).

It must be acknowledged, however, that in common with his generation he believed in witches and evil spirits, and he was thoroughly non-advanced in his attitude towards women. He does not hesitate, for example, to assert that there are more witches than wizards (77), and he endows the humble flea with a spirit of the feminine gender : « A flea is but a little best, yet if she were not possest with a spirit, she could never leape and skip so as she doth » (78). Finally, he gives up trying to explain the haunting of men by their wives after death, «... whereof I can gather no reason but this, that Women are borne to torment a man both alive and dead » (79).

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(73) *Strange newes...* 1592. Preface. (Works, ed. by McKerrow, I.259.)
(74) Works, ed. by McKerrow, I.365.9-13; 366, 22-26.
(75) *Ibid.*, I.356.28-29.
(76) *Ibid.*, I.370. 14-25.
(77) *Anatomy of absurditie.* 1589. (Works, ed. by McKerrow, I.16.30.)
(78) *Terrors of the night.* 1594. (Works, ed. by McKerrow, I.350.)
(79) *Ibid.*, I.383.26-28.

Chapter V

ALCHEMY

> « Methinks the Chymists in their search after truth are not unlike the navigators of Solomon's Tarshish Fleet, who brought home from their long and perilous voyages, not only gold and silver and ivory, but apes and peacocks too... »
>
> Robert Boyle. (80)

In the century under review there was as yet no real science of chemistry, and no scientific medicine. Doubtless some measure of honest inquiry still prevailed among the alchemists who were attempting cures by a crude sort of medical chemistry. But the references in contemporary literature yield such a picture of quackery and gullibility as is hard to reconcile with the intellectual spirit of the age and its practical achievements in commerce and in war. Many of these allusions are little more than jargon to the modern reader, and more than one of the Elizabethan dramas would be unintelligible without an astrological and alchemical glossary.

There is comparatively little use of alchemical words and phrases in their technical sense in Shakespeare, but the multiplying and transforming feature of the art is effectively employed both in descriptions of natural scenes and as a figurative symbol of human attributes.

> ... the glorious sun
> Stays in his course and plays the alchemist,
> Turning with splendour of his precious eye
> The meagre cloddy earth to glittering gold (81).

The Sonnets speak of « Gilding pale streams with heavenly alchemy (82) » and of the alchemical power of love,

> To make of monsters and things indigest
> Such cherubins as your sweet self resemble.
> Creating every bad a perfect best
> As fast as objects to his beams assemble (83).

(80) *The sceptical chymist*. (Works, ed. by Birch, ed. of 1772. V. 1, p. 584.)
(81) *King John*, III.1.77-80.
(82) Sonnet XXXIII.
(83) Sonnet CXIV.

while it is said of Brutus :

> And that which would appear offence in us,
> His countenance, like richest alchemy,
> Will change to virtue and to worthiness. (84)

Although so widely practiced, and not without royal encouragement in the time of ELIZABETH, alchemy was actually illegal in England throughout this period, and therefore no serious treatises on the subject in English are to be found. Real practitioners of the art, such as JOHN DEE and his one-time partner, EDWARD KELLY (85), are nevertheless openly referred to in current writings, and a letter from RICHARD EDEN to LORD BURGHLEY reports the following interesting but rather indefinite experiment :

> And that this greate and divine secreate of this Michrocosmos maye not seeme incredible unto your honour, I assure you that I, that am *minimus philosophorum*, dyd long sense... woorke and secretly practise sumwhat like unto this... : I dissolved two substances in two waters; then I put the waters togyther in a glasse, suffering them so to remayne for a tyme. Then I stilled off the water from the masse or chaos lefte of them bothe, and put it on agen, and so dyd dyvers tymes. In fine, the masse being dissolved in the water, I let it rest all night in a coulde place. In the morning, I founde swymming on the water and in the myddest thereof a little round iland, as brode as riall or sumwhat more, with at the least a hundreth sylver trees about an ynche high, so perfectly formed with trunkes, stalkes and leaves, all of most pure and glystering sylver, that I suppose no limner or paynter is able to counterfecte the like. Then shaking the glasse, all fell in pieces into the water, and filled it with glystering sparkes, as the firmament shyneth with starres in a cleare winter night. Then putting the glasse to a softe fyre upon warme asshes, all turned agen into clear water, which, agen being put in a colde place all night, made an island with the like trees as before (86).

In one of his attacks upon HARVEY, NASHE charges : « It is not unknown to report... how many pounds you have spent (and as it were, throwen into the fire) upon the durt of wisedome called Alcumie » (87). He refers to the alchemists as « the forenamed

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(84) *Julius Caesar*, I.3.158-160.

(85) « Let him call upon Kelly... and for the spirites and soules of the ancient Alchumists. » NASHE. *Have with you to Saffron-Walden*. 1596. (Works, ed. by MCKERROW, III.52.3-5.)

(86) HALLIWELL-PHILLIPPS, *op. cit.*, pp. 4-5.

(87) *Strange newes*... Epistle dedicatorie. (Works, ed. by MCKERROW, I.255.21-24.)

Gold-falsifiers » (88), and, scoffing at their pretended control over departed spirits, asserts :

> Whether you call his fire Purgatorie or not, the fire of Alchumie hath wrought such a purgation or purgatory in a great number of mens purses in England that it hath clean fir'd them out of al they have (89).

It is BEN JONSON, however, who thoroughly satirizes the charlatans of the day and the universal weaknesses of human nature in his two dramas, *The Alchemist* and *Mercury Vindicated*. All ranks of society are shown to have been imbued with the greed for easy gold. Not only Draper, the law clerk, who would have « a familiar to rifle with at horses, and win cups » (90); and Drugger, a seller of tobacco, who « would know by art, sir, of your worship, which way I should make my door, by necromancy, and where my shelves; and which should be for boxes and which for pots » (91); but Sir Epicure Mammon, duped for months, and returning to be fleeced again, seem to indicate that the « art of connycatching » was almost an evocation of the law of supply and demand.

> ... This night I'll change
> All that is metal in my house to gold :
> And, early in the morning, will I send
> to all the Plumbers and the pewterers,
> And buy their tin and lead up; and to Lothbury
> For all the copper... (92)

To his companion, *Surly*, « Who is, indeed, sir, somewhat costive of belief... and would not be gull'd », Sir Epicure promises this uncontestable evidence :

> I'll show you a book where Moses and his sister
> And Solomon have written of the art;
> Ay, and a treatise penn'd by Adam...
> I have a piece of Jason's fleece too, which was
> no other than a book of alchemy. (93)

(88) *Ibid.* Epistle to the gentlemen readers. (Works, ed. by McKERROW I.261.8.)
(89) *Have with you to Saffron-Walden.* (Works, ed. by McKERROW, III.52.9-12.)
(90) *The alchemist.* 1612. I.1. (Harvard classics, v. 2, p. 528.)
(91) *Ibid.*, I.3. (Harvard classics, v. 2, p. 536.)
(92) *Ibid.*, II.1. (Harvard classics, v. 2, p. 541.)
(93) *Ibid.*, p. 543.

Almost equally interested in the great medicinal virtues of « the stone », the deluded victim goes on :

> . . . . . 'Tis the secret
> Of nature naturiz'd 'gainst all infections,
> Cures all diseases coming of all causes;
> A month's grief in a day, a year's in twelve;
> And, of what age soever, in a month;
> Past all the doses of your drugging doctors.
> I'll undertake, withal, to fright the plague
> Out o' the kingdom in three months...
>
> Do you think I fable with you? I assure you
> He that has once the flower of the sun,
> The perfect ruby, which we call elixir,
> Not only can do that, but by its virtue,
> Can confer honour, love, respect, long life;
> Give safety, valour, years, and victory
> To whom he will. In eight and twenty days,
> I'll make an old man of fourscore, a child.

to which the sceptical *Surly* cynically replies : « No doubt; he's that already » (94).

Perhaps the world is always « full of fooles and full of sectes » (95), as GABRIEL HARVEY said of his own age, « wherein the spirit of contradiction reigneth » (96). Yet surely at no period has there been a greater contrast between two departments of the national mind than between Elizabethan science with its heavy overlay of astrology and alchemy and the golden age of English literature.

CHAPTER VI

MAPS AND MARITIME MATTERS

> « If in Naples
> I should report this now, would they believe me? (97) »

After a century of exploration, and following the publication of several important English works on navigation, there appeared in 1600, drawn by EDWARD WRIGHT, the first map of the world

(94) *Ibid.*, p. 542.
(95) Sonnet XIX. (Works, ed. by GROSART, v. 1, p. 250.)
(96) *Foure letters...* (Works, ed. by GROSART, v. 1, p. 203.)
(97) *Tempest* III.3.39-40.

to be published in England on the modern principles of projection. Hazy ideas had hitherto prevailed as to the non-European world « of islands far away », and only by slow degrees were the various travellers' tales fitted together to form the islands into continents. Sir WALTER RALEIGH refers to « the fictions, or let them be called conjectures, painted on maps, to serve only to mislead such discoverers as rashly believe them, drawing upon the publishers either some angry curses or well-deserved scorn ». In support of which view he tells the following story :

> ... I remember a pretty jest of Don Pedro de Sarmiento, a worthy Spanish gentleman who had been employed by his king in planting a colony upon the Straits of Magellan : for when I asked him, being then my prisoner, some questions about an island in those straits, which methought might have done either benefit or displeasure to his enterprise, he told me merrily, that it was to be called the Painter's Wife's Island; saying that whilst the fellow drew this map, his wife sitting by desired him to put in one country for her, that she in imagination might have an island of her own (98).

Among the map-makers was the versatile JOHN DEE, who, in 1580, provided two ship-masters with a « new charte (made by hand) », directing their voyage to Cathay « more exactly than any other yet published » (99). Even so were they advised upon their arrival to note down the situation of cities within the land, « ever assaying to come by some chartes or maps of the cuntry made and printed in Cathay or China; and by some of their bookes likewise, for language » (100).

Great progress had meanwhile been made in the art of navigation. In 1600, after eighteen years of experiment, GILBERT published his *De Magnete*, with a laudatory preface by the same EDWARD WRIGHT before mentioned, who, as a practical navigator, knew the worth of « the airy fables » so little valued by Chancellor BACON. Nevertheless, this epochal book, acknowledged « as the greatest and best in magneticall philosophy » (101), did not at once put to flight ancient superstitions regarding magnetism and the exaggerated powers of the loadstone.

(98) *History of the world*. Bk. II, ch. XXIII. (Works. Oxford Press, 1729. V. 4, p. 684.)

(99) HALLIWELL-PHILLIPPS, *op. cit.*, p. 20.

(100) *Ibid.*, p. 21.

(101) RIDLEY, MARK. *A short treatise of magneticall bodies and motions*. London, 1613. Preface magneticall.

An English translation of PLINY, brought out in 1601 (102), relates among other wonders that DINOCRATES began to make the arched roof of the temple of Arsinoe all of loadstone, which is probably the source of that oft-repeated fable of the « statue in mid-air ». Among the « commonly presumed truths » rejected by Sir THOMAS BROWNE is the drawing power of the legendary magnetical rocks that were said to pull out iron nails, and to pass which, therefore, ships must be fitted with wooden pegs (103). BURTON speaks of « Many greene wounds... magnetically cured » (104), and refers to « Balsomes, strange extracts, elixars, and such like magico-magneticall cures » (105), questioning « whether by these diabolicall meanes... this disease and the like may be cured? and if they may, whether it bee lawfull to make use of them? (106) »

The adjective magnetic is used by BEN JONSON in the title of one of his plays (107), but is strangely absent from the SHAKESPEARE vocabulary, although figuratively employed by others in true Shakespearean fashion.

From Quarles' Emblems comes the simile,

« Like as the am'rous needle joys to bend to her magnetic friend (108). »

Sir THOMAS HERBERT speaks of « Turk, Jew, and others, drawne thither by the magnetick power of gaine » (109), and Rowland declares : « There is such a magnetick force in Goodness, that it draws the hearts of men after it » (110).

Following the new knowledge of magnetism, navigation was further aided by the perfecting of the compass, as well as by the invention of the dipping-needle (inclinatorium) and other « ingenious and necessarie instruments for sea-men to find out...

(102) *The Historie of the world;* tr. by P. HOLLAND.
(103) *Pseudodoxia epidemica.* Bk. II, ch. 3. (Works, ed. by WILKIN, v. 1, pp. 144-147.)
(104) *Anatomy of melancholy.* Pt. 1, Sec. 2, Mem. 3, Subsec. 2. (Ed. of 1632, p. 94.)
(105) *Ibid.*, Pt. 2, Sec. 2, Mem. 3. (Ed. of 1632, p. 281.)
(106) *Ibid.*, Pt. 2, Sec. 1, Mem. 1, Subsec. 1. (Ed. of 1632, p. 219.)
(107) *The Magnetick lady.* London, 1632.
(108) I.XIII. (Ed. of 1718, p. 53.)
(109) *Travels into Africa and Asia.* Bk. II. (Ed. 2, 1638, p. 155.)
(110) TOPSELL's *History of four-footed beasts.* London, 1658. Preface.

the latitude of any place upon the sea or land, in the darkest night that is, without the helpe of sunne, moone, or starre » (111). And, accompanying these substantial contributions to science, were many suppositious inventions, including several marine appliances of astonishing powers.

EDMUND JENTILL, a counterfeiter (though only « of Forren coyne not currant in this realme »), appeals to Lord BURGHLEY for release from prison, proposing in redemption of his fault (112):

> A device whereby twoe menne maye be sufficient to waye the waytiest anker in her Majestie's navye.
>
> A device wonderfully strange... whereby a vessell of burden may easilye and safely be guided both against the winde and tide.

RALPH RABBARDS also offers the Queen (113) :

> The rarest engyne that ever was invented for sea service : A vessel in manner of a galley or galliotte to passe upon the seas and ryvers without oars and sayle, against wynde and tyde, swifter than any that ever hath bynne seene, of wonderfull effect bothe for intelligence, and many other admirable exploytes, almost beyonde the expectation of man.

Although no detailed designs are attached to any of these proposals, there is, in fact, a fourteenth century manuscript showing the drawing of a boat with paddle-wheels (114), and earlier still, as told in the *Famous Historie of Friar Bacon*, a confident prophecy declared : « There may be made instruments of navigation without men to rowe in them, as great ships to brooke the sea, only with one man to steere them, and they shall sayle far more swiftly than if they were full of men » (115).

The Utopians are reported to « have also wittelye excogitated and devised instruments of divers fassions, wherein is exactly comprehended and conteyned the mouynges and situations of the sonne, the mone, and of al the other starres, which appere

(111) In *The theoriques of the seven planets* (1602), THOMAS BLUNDEVILLE acknowledges his indebtedness to GILBERT for these instruments. (See American institute of electrical engineers. Catalogue of the Wheeler gift, p. 89.)

(112) HALLIWELL-PHILLIPPS, *op. cit.*, pp. 35-36.

(113) *Ibid.*, p. 11.

(114) *Ibid.*, p. 11, footnote.

(115) *Collection of early prose romances*, ed. by W. J. THOMS. London, 1828, pp. 24-25.

in theire horizon » (116), their advance over contemporary Englishmen being indicated by the acceptance of natural phenomena for what they were :

> But as for the amityes and dissentions of the planettes, and all that deceyteful divination by the starres, they never as much as dreamed thereof. Raynes, windes and other courses of tempestes they knowe before by certein tokens, which they have learned by long use and observation (117).

To this account of practical accomplishment More adds the « nipping taunte » :

> Of all these Philosophers, whose names be heare famous in this parte of the worlde to us knowan, before our cummynge thether not asmuche as the fame of annye of them was cumen amonge them. And yet in Musike, Logike, Arythmetyke, and Geometrye they have founde oute in a manner all that oure ancient Philosophers have tawghte. But as they in all thinges be almost equal to our olde auncyente clerkes, so oure new Logiciens in subtyle inventions have farre passed and gone beyonde them. For they have not devysed one of all those rules of restrictions, amplifications, and suppositions, verye wittelye invented in the small Logicalles, whyche heare oure children in every place do learne... (118)

It is admitted, however, that the narrator « founde great favoure and friendship among them, for teachynge them the feate and the use of the lode stone. Which to them before that time was unknowne. And therefore they were wonte to be verye timerous and fearfull upon the sea, nor to venter upon it but only in the somer time. But nowe they have suche a confidence in that stone, that they feare not stormy winter... In so muche, that it is greatly to be doubted lest that thyng, throughe their owne folish hardinesse, shall tourne them to evyll and harme, which at the first was supposed shoulde be to them good and commodious » (119).

Unlike good King Utopus, the ruler of the *New Atlantis* (120) had nothing to learn from European travelers. On the contrary, it is they who are lost in wonder as he courteously discloses for their enlightenment the superior inventions of the « College of

(116) More, Sir Thomas. *Utopia*. London, 1551. Bk. II. (English reprints, ed. by E. Arber, p. 105.)
(117) *Ibid.*, p. 105.
(118) *Ibid.*, pp. 104-05.
(119) *Ibid.*, Bk. I, p. 32.
(120) Published with *Sylva sylvarum* in 1627.

the Six Days Works ». In this report of the wonders of Solomon's House is perhaps our best picture of what was hoped and expected of English science in the early seventeenth century, but still a long way from being practically realized.

They had tall towers, half a mile high, placed on hills for the view of divers meteors, and also used for isolation and refrigeration. Perspective houses were provided for demonstrations of light and radiations; means also for « seeing objects afar off, as in the heavens, and remote places », in addition to glasses to see minute bodies perfectly—shapes and colors of small flies, grains and flaws in gems, observations in urine and in the blood. There were « chambers of health » with air qualified to cure divers diseases; artificial wells and fountains, one called water-of-paradise being « by that we do to it very sovereign of health and prolongation of life » (121).

Other wonderful contrivances, long dreamed of in Europe, were in common use among them. « We have some degrees of flying in the air : we have ships and boats for going under water, and brooking of seas... We have divers curious clocks... and some perpetuall motions ». These marvels were no chance discovery, but the result of a complex research organization outrivaling the twentieth century.

It was arranged that two ships should periodically set forth, from which three fellows or brethren should be landed to give knowledge of the countries visited « and especially of the sciences, arts, manufactures, and inventions of all the world; and withal to bring unto us books, instruments, and patterns in every kind ». The ships trade their treasure and return, but the brethren stay abroad until the arrival of the new mission twelve years later. A highly specialized staff is trained to examine and correlate the data thus gathered. The « traveling fellows » who go to foreign countries are called « Merchants of Light »; those who collect experiments are named « Mystery Men »; they who try out the new experiments are « Pioneers » or « Miners »; « Dowry Men » or « Benefactors » are they who seek good applications of these experiments; others that direct new experiments more penetrating

(121) Description of Solomon's House, pp. 31-43. (Harvard classics, v. 3, pp. 181-89.)

than the former, « these we call ' Lamps ' »; lastly, they only that raise the former discoveries into greater observations, axioms and aphorisms are known as « Interpreters of Nature » (122).

CHAPTER VII

SCIENCE IN WAR AND IN PEACE

> « The wittes therefore of the Utopians inurede and exercised in learnynge, be marveilous quick in the invention of feates helping annye thinge to the advantage and wealthe of lyffe. »
>
> Sir THOMAS MORE (123).

It seems to have been the opinion of the practical-minded GABRIEL HARVEY that imagination outran achievement in the scientific world of his time, for he caustically inquires :

> ... how cometh it to passe, that much more is professed, but much lesse perfourmed, than in former ages? especially in the Mathematikes and in naturall Magie; which being cunningly and extensively imployed... might wonderfully bestead the Commonwealth : with many puissant engines, and other commodious devises, for warre, and peace... (124)

Certainly, it must be acknowledged that speculation was long and deliberate, and that men's minds were occupied with theories, new and old, and not overcritically tested. The doctrine of refractions and reflections was long one of the favorite topics of conversation and of correspondence, to which numerous allusions attest. But we need to remember that after these amateur experiments on « perspective glasses » came the telescope, and following the tables so slowly and laboriously worked out, came Sir ISAAC NEWTON and the modern science of optics.

Inventive genius would naturally turn its attention towards the golden opportunities in the military field throughout that turbulent period. In the time of ELIZABETH, RALPH RABBARDS, a name unknown to later fame, offers the Queen « at as smalle chardge

(122) Account of the research staff, pp. 43-45. (Harvard classics, v. 3, pp. 189-190.)

(123) *Utopia*. *Op. cit.*, p. 120.

(124) *Foure letters*. (Works, ed. by GROSART, v. 1, pp. 299-30.)

and to as greate purpose as any other ingeniors or practitioners of Christendome », the following remarkable devices (125):

A flying fire which shall, without ordynance, and farre of, wonderfully annoye any battayle, towne, or campe, and disperse even as if it did rayne fire... very terrible both to men and horse.

A shotte for greate ordynance to pierce deeper then any other shotte, and sett on fire whatsoever it strike through or sticketh in. A most noble ingen, specially for sea service.

A firy chariott without horses to runne upon the battaile and disorder it, that no man shal be able to abide or come nighe the same, and wil be directed even as men will to tourne, to staye, or come directly backe upon any present danger, or elles to followe and chase the enemye in their flighte.

A means to better the use of small artillery... where 1000 are nowe shotte, and not 10 men fall, it will appeare, by good demonstration and experience, that 10 shotte of 1000 shall hardly misse.

HENRY MARSHALL, too, is prepared to supply Lord BURGHLEY with these rare implements :

1. An engine whereby the walls of annye towne or castle may be defended from the force of anye canon.

2. An engine which shall breake the araye of anye battell being ready to joyne,

« the which engines are easilie made, and yett neither very chargeable nor paynefull to transporte » (126).

MORE speaks of the armour of the gentle Utopians, « whiche when they be made they kepe very secrete, leaste yf they should be knowen before neade requyre, they should be but laughed at and serve to no purpose » (127). Nor were engines of war absent from BACON's ideal commonwealth, whose citizenry boast of new mixtures and compositions of gunpowder, as well as « instruments for all sorts of motions, swifter than any, stronger and more violent, exceeding your greatest cannons and basilisks » (128).

Other interesting benefactions offered for the royal consideration were distilled waters « for medicyne inward or outward... one spoonful better than a gallon of other »; especially a water for the eyes « as well for preservinge and comfortinge the sighte,

(125) HALLIWELL-PHILLIPPS, *op. cit.*, pp. 7-8.
(126) *Ibid.*, p. 37.
(127) *Utopia*. *Op. cit.*, p. 141.
(128) *New Atlantis*, p. 42. (Harvard classics, v. 3, p. 188.)

as to restore that which is lost » (129). Hence it appears that the arts of peace were simultaneously claiming the attention of science.

Mechanical improvements were meanwhile appreciatively welcomed, if we may judge by NASHE's generous suggestion : « Amongst all other stratagems and puissant engins, what say you to Mates Pumpe in Cheapside, to pumpe over mutton and porridge into Fraunce ? this colde weather our souldiors, I can tell you have need of it... » (130)

Current ideas concerning these mechanical powers and motions were assembled by JOHN WILKINS, Bishop of Chester, in a small volume entitled, *Mathematical Magick : or the Wonders that may be Perform'd by Mechanical Geometry* (131). This discourse, promising delight and pleasure to every rational reader, is particularly recommended alike to common artificers « who may be much advantaged by the right Understanding of their Grounds and Theory », and to Gentlemen engaged in various « chargeable Adventures » on their estates, who will « thereby more easily avoid the Delusions of any cheating Impostor » (132). As an illustration of contemporary opinion, a few chapter headings may be quoted :

> Concerning the infinite Strength of Wheels, Pulleys, and Screws; that it is possible by the Multiplication of these, to pull up any Oak by the Roots with a Hair, lift it up with a Straw, or blow it up with ones Breath; or to perform the greatest Labour with the least Power.
>
> Of the Moveable and Gradient Automata representing the Motions of Living Creatures, various Sounds of Birds, or Beasts, and some of them articulate.
>
> Concerning the Possibility of Framing an Ark for Submarine Navigations...
>
> Concerning the Art of Flying. The several Ways whereby this hath been, or may be attempted.

This admirable little book at the same time reviews the various schemes proposed to effect perpetual motion—by chemistry, through magnetism, by solid and fluid weights, accompanied by well-balanced remarks upon « the seeming facility and real difficulty

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(129) HALLIWELL-PHILLIPPS, *op. cit.*, p. 8.
(130) *Strange newes.* (Works, ed. by MCKERROW, I.331.28-31.)
(131) Published in 1648.
(132) Preface.

of any such contrivance... with the fallibleness of it upon experiment » (133).

A hint of highly expert craftsmanship is contained in the following tale (from HOLINSHED's Chronicle) explaining a curious allusion by NASHE to « tying a flea in a chaine » (134) :

> One Mark Scaliot, blackesmith citizen of London... for triall of workemanship... made a chaine of gold of three and fortie linkes, to the which chaine the locke and keie being fastened, and put about a fleas necke, she drew the same with ease. All which locke, keie, chaine, and flea weied but one graine and a halfe (135).

No marked achievements in the field of improved communication can be credited to this period. The « Sympathetic Telegraph, » so long believed to be a possibility through the effect of one dialled magnet upon another, was at last acknowledged to be inoperative at any considerable distance, and was included by Sir THOMAS BROWNE among the *Vulgar Errors* not to be accepted. As early as 1603, however, a book of inventions dedicated to King JAMES lists a device « to convey the voice for a thousand paces without showing any one near you were talking » (136). If not a promise of the modern telephone, this was at least a precursor of the « Otacousticon » exhibited at the Royal Society some sixty years later, concerning which PEPYS writes (137) :

> « ... I did try the use of the Otacousticon, which was only a great glass bottle broke at the bottom, putting the neck to my ear, and there I did plainly hear the dancing of the oars in the boats in the Thames to Arundel gallery window, which, without it, I could not in the least do. »

To complete the picture here afforded, it will be interesting to observe the relations between religion and science in that controversial day. There would naturally be less actual union between the two in Protestant England than in the monasteries of Catholic Europe, from whence came so many of the advances in medieval science. Nevertheless, it is easy to prove by abundant citation

(133) See headings to Chapters IX and XV.

(134) « ... hee hath vowd... to discover and search foorth certaine rare Mathematicall Experimentes; as for example, that of tying a flea in a chaine. » *Have with you to Saffron-Walden*. (Works, ed. by MCKERROW, III.37.15.)

(135) *Holinshed's chronicle*. (Ed. of 1587, v. 2, p. 1299.)

(136) HALLIWELL-PHILLIPPS, *op. cit.*, Preface XIII, footnote.

(137) *Diary*. April 2, 1668.

that the human reaction to new theories was the same in both lands, and that these responses are, indeed, independent of time and of place. As Bishop WILKINS points out, the « tying of the Meaning of Scripture to the Letter of it, and from thence concluding Philosophical Points » (138), is an ancient, if not a universal, tendency of certain types of minds. In commenting upon restrictive measures then current, he remarks : « And it seems the Fathers of the Council of Trent were not such confident Defenders of PTOLEMY's hypothesis against COPERNICUS, as many now are » (139).

Typical of the best opinion at the beginning of our period, we may quote the Catholic MORE, and for the later years, the views—so strikingly similar in spirit and tone—of that good Protestant, THOMAS BROWNE. Thus, the Utopians are seen to engage in the contemplation of nature not only for their own « wonderfull greate pleasure »,

> ... but also to obteine great thankes and favour of the autour and maker thereof. Whome they thinke according to ye fassion of other artificers, to have set forth the marvelous and gorgious frame of the world for man with great affeccion intentively to beholde. Whom only he hath made of witte and capacitie to consider and understand the excellencie of so great a woork. And therefore he beareth (say they) more goodwill and love to the curious and diligent beholder and vewer of his woork and marvelour of the same, than he doth to him, which like a very brute beaste without witte and reason, or as one without sense or moving, hathe no regarde to soo greate and soo wonderfull a spectacle (140).

With equal force, the *Religio medici* declares :

> The wisdom of God receives small honour from those vulgar heads that rudely stare about, and with a gross rusticity admire his works : those highly magnify him, whose judicious inquiry into his acts, and deliberate research into his creatures, return the duty of a devout and learned admiration... (141)

> And thus I call the effects of nature the works of God, whose hand and instrument she only is; and therefore to ascribe his actions unto her, is to devolve the honour of the principal agent upon the instrument, which if with reason we may do, then let our hammers rise up and boast that they have built our houses, and our pens receive the honour of our writing (142).

(138) *Discourse concerning a new planet*. Prop. I. (Mathematical and philosophical works. Ed. 1708, p. 156.)
(139) *Ibid.*, p. 156.
(140) *Utopia*, Bk. II. *Op. cit.*, pp. 119-20.
(141) Pt. 1, Sec. XIII. (Works, ed. by WILKIN, v. 2, p. 338.)
(142) *Ibid.*, Sec. XVI, p. 342.

What better conclusion to the unending discussion—as applicable now as then—than this seasoned affirmation of a later poet:

> « Nature is but a name for an effect
> Whose cause is God » (143).

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(143) COWPER's *Task*, Bk. VI. (Quoted in Boston, 1889, ed. of *Religio medici*, p. 34, footnote.)

John Bird (1709-1776)

Mathematical Instrument-Maker in the Strand

1. The progress of practical astronomy depends in a great measure on the accuracy of the equipment with which the astronomers work. Consequently, the names of the makers of astronomical instruments should be rescued from the oblivion into which neglect is throwing them, and the men should be honored in accordance with the merit of their work. During their life-times these men often filled important positions and commanded both attention and respect. The scientific world recognized their abilities and called upon them for advice and assistance. Often they were the collaborators and intimate associates of renowned astronomers.

2. JOHN BIRD was an eminent mathematical instrument-maker. known throughout Europe for the accuracy of his instruments. (1) His advent on the scene of English instrument-making was a fortuitous one, and his efforts were furthered by the advice and instruction of GEORGE GRAHAM.

3. BIRD was born in Bishop Auckland in the County of Durham, and was a friend of WILLIAM EMERSON, the mathematician, of Hurworth, Durham County, and of JEREMIAH DIXON of the parish of Cockfield, in the same county. BIRD had some connection with Woolwich Academy, the Royal Military School, which enabled him to recommend DIXON as the person to be sent to St. Helena to make observations of the transit of Venus. (2) This same DIXON, with CHARLES MASON, later was sent to America to measure

(1) *Gentlemen's Magazine, (Gent. Mag.)*, XLVI, 192, 1776.

(2) FORDYCE, WILLIAM, *The History and Antiquities of the County Palatine of Durham;...*, I, 558 footnote (Newcastle). For EMERSON see I, 499-501 and also the *Dictionary of National Biography (D. N. B.)*.

the boundary between Pennsylvania and Maryland, whence the common reference to the « Mason and Dixon Line ». (3) According to LUDLAM (4), BIRD prepared for the profession of a cloth weaver. In a clock-maker's shop he accidentally observed the irregular divisions of the minutes and seconds on a clock dial plate, and was offended by them. This incident first turned his attention to the art in which he later excelled. With great neatness and accuracy, he himself divided another plate, and for some time amused himself by dividing dial plates for the clock-maker, engraving all the figures on his own instruments. About 1740 he went to London, and by 1745 was in business for himself. In 1740 he invented an instrument for finding the latitude at sea. (5)

4. In the capital he divided instruments for SISSON, the well known instrument-maker, and the neatness and accuracy of his work attracted the attention of GRAHAM, of whose friendly communications he availed himself, and on whose work he later improved. (6)

5. LUDLAM says that GRAHAM had both a skill in practical mechanics and a love of astronomy and that he brought such perfection into the art of constructing and graduating instruments that a new era in astronomy dates from his time. (7) LUDLAM says it was important for the public that BIRD arrived on the scene towards the end of GRAHAM's life and was peculiarly endowed to supplement the latter's work in dividing and constructing

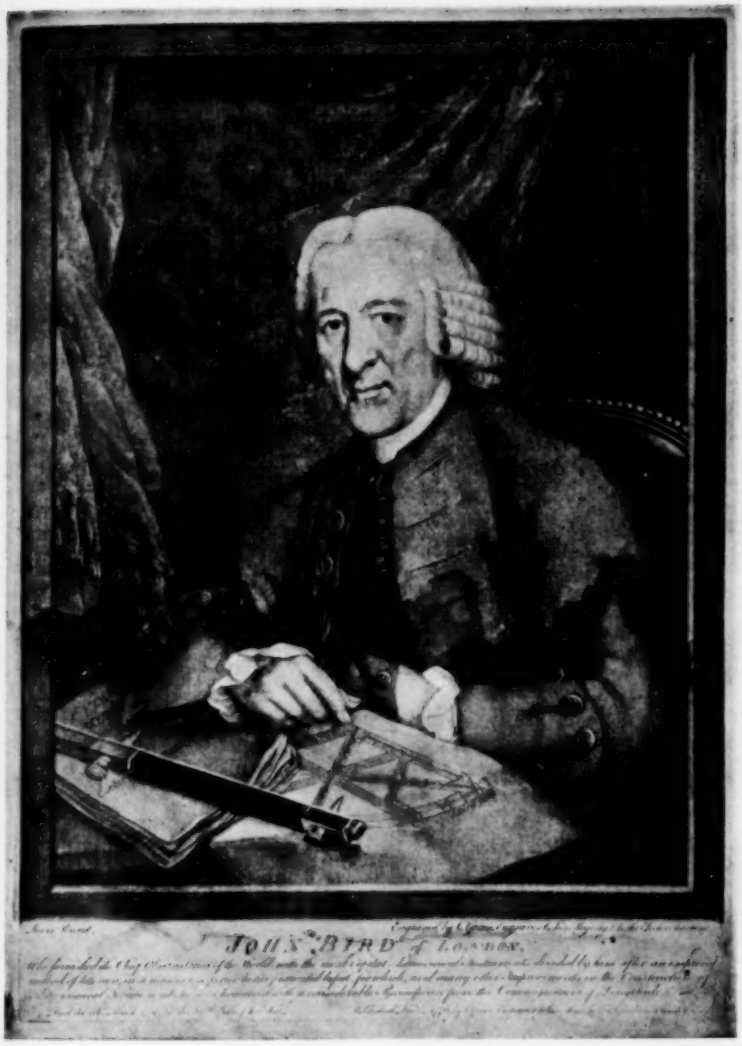
(3) This subject will be brought up later in connection with the instruments BIRD made for the undertaking. DIXON is not mentioned in the *D. N. B.*, except in the article *Mason, Charles*, nor in the new dictionary of American biography. Something of his early life can be found in FORDYCE, W., *op. cit.*, II, 78-79 footnote. Those who wrote on him as the surveyor seem not to have known of this. The story of his birth in a coal mine is scarcely credible.

(4) LUDLAM, W., *An Introduction and Notes, on Mr. Bird's Method of Dividing Astronomical Instruments. To which is added, a Vocabulary of English and French Technical Terms*, preface iv, note (London 1786).

(5) FORDYCE, W., *op. cit.*, I, 558 footnote and SYKES, JOHN, *Local Records...* [of Durham etc.] I, 165 (Newcastle 1833).

(6) LUDLAM, W., *op. cit.*, preface iv.

(7) *Ibid.*, preface iii. For further information about GEORGE GRAHAM see my essay in *Popular Astronomy*, April 1931 and the more complete version in the *Vassar Journal of Undergraduate Studies*, May 1931.



Isis, XVII, pl. 8 JOHN BIRD C. D. HELLMAN.
1709-1776
Painter: LEWIS (? JOHN, F. S. A., fl. 1762-1776)
Engraver: VALENTINE GREEN, 1739-1813.
By courtesy of Sir FRANK DYSON, Astronomer Royal,

Isis, XVII, pl. 9. GEORGE GRAHAM C. D. HELLMAN.

1673-1751

Painter: THOMAS HUDSON, 1701-1779

Engraver: JOHN FABER, the younger, 1695?-1756.

By courtesy of Sir FRANK DYSON, Astronomer Royal.

astronomical instruments. (8) RIGAUD, in his famous BRADLEY memoirs, also stresses this point. (9)

6. It does not seem that BIRD was as proficient an astronomer as GRAHAM. At least, there are but few observations by BIRD on record, and while those few testify to his knowledge of the theory of observing, they do not point him out as a skilled observer. The first of these which I have found is the transit of Venus on June 6, 1761 communicated to the Royal Society by NATHANIEL BLISS in a letter to the Right Honourable GEORGE Earl of MACCLESFIELD. In this letter BIRD is listed as having observed the transit of Venus, using a reflector of 18 inches focal length of his own making. BLISS also speaks of a 12-foot telescope and micrometer made by BIRD and used by HORNSBY for similar observations at Shirburn. (10) BLISS again mentioned BIRD in connection with the solar eclipse of April 1, 1764, when BIRD used a 2-foot reflecting telescope and when BIRD and BLISS measured the moon's diameter. (11)

7. Beyond this, references to BIRD merely concern his mechanical skill. HORNSBY, the Savilian Professor of Astronomy, a fellow of the Royal Society, communicated a paper to that society on the proper motion of Arcturus and on the obliquity of the ecliptic. In doing this, he had occasion to refer to BIRD's services at Shirburn. In 1743 that observatory was using a mural quadrant of five French feet, constructed by SISSON. The body of this quadrant was not sufficiently strong and the linear divisions were less accurate than expected. So in the summer of 1745, after the body of the instrument had been strengthened, the Earl of MACCLESFIELD employed BIRD to put a set of points on the limb. (12) According to RIGAUD, the quadrant bore GRAHAM's division of 96 as well as that of 90°. (13) BIRD began by determining the 96 parts and subdividing each into 8. (14) In June

(8) LUDLAM, W., *op. cit.*, preface iii.

(9) RIGAUD, STEPHEN PETER, *Miscellaneous works and correspondence of the Rev. James Bradley, D.D. F.R.S.*, lxxvi (Oxford 1832).

(10) *Philosophical Transactions (Phil. Trans.)*, LII part I, 175-176.

(11) *Ibid.*, LIV, 142-143.

(12) *Ibid.*, LXIII, 93 ff.

(13) RIGAUD, S. P., *op. cit.*, lxxxiii.

(14) *Ibid.*, lxxxiv.

1751, he bisected the distances between the points he had added, the line of collimation being found to have varied, making the zenith distances too small from the end of 1746 to June 1751. (15) After BIRD's divisions had been added the quadrant was placed on a table, and SISSON's quadrant was found to be 10″ less than 90°. (16) The beam compass, adjusted to 60°, was applied to a number of BIRD's points, none of which were found to vary from their true positions by more than one second. Quoting Lord MACCLESFIELD, RIGAUD gives the date of July 22, 1751 for BIRD's insertion of the intermediate divisions. The quadrant was so arranged that it could be moved to the opposite sides of the pier on which it was hung. (17)

8. About this time BIRD was called upon to provide some instruments for BRADLEY at the Royal Observatory. From 1742 to 1750 BRADLEY had contented himself with the same instruments HALLEY had used (GRAHAM's iron 8-foot mural quadrant). BIRD made him a brass mural quadrant of 8-foot radius and a transit instrument of 8-foot focal length. With these instruments BRADLEY began a series of observations of the sun, moon, planets, and fixed stars, which was carried on by others after his death. In 1776 MASKELYNE published the Greenwich observations for 1765 to 1774 and said of BIRD's instruments, then still in use, that « the exactness of the instruments is so great, and their rectifications so nice, that the place of any heavenly body may be always found by them within ten seconds of a degree, both in Longitude and Latitude, and generally much nearer. » (18)

9. GRAHAM's instruments had been remarkable, testifying to a true mechanical genius and a knowledge of practical astronomy, but improvements in astronomical instrument-making had been devised in the twenty-five years that had passed since GRAHAM's construction of BRADLEY's instruments, and the latter's request to the King for better ones and for a more convenient building

(15) *Phil. Trans.*, LXIII part I, 110.

(16) RIGAUD, S. P., *op. cit.*, lxxxiii.

(17) *Ibid.*, lxxxiv and note. According to the *D. N. B.*, XV, 234, the Shirburn Castle observing books are in the Savilian Library at Oxford.

(18) MASKELYNE, the Reverend NEVIL, B. D., *Astronomical Observations made at the Royal Observatory at Greenwich, from the year MDCCLXV to the year MDCCLXXIV*, I, preface i (London 1776).

to house those used for meridian observations was not an unreasonable one. The grant of His Majesty as a result of this application made BIRD's work possible. This grant provided for a transit instrument with an 8-foot telescope, having an axis of 4½ feet and for an 8-foot quadrant, of the size made by GRAHAM in 1725, and also for other instruments which might help to advance astronomical knowledge. (19) At the completion of the new building, the Royal Observatory was provided with a moveable quadrant of 40-inch radius, made by BIRD. (20)

10. The work of alteration and new construction began in 1746 as is shown by the schedule of expenses given below, although the year 1750 is usually given as the year of the renovation of the observatory and its instruments. (21) RIGAUD gives the year 1748 for the grant of the £ 1000 from the king (GEORGE II) and WELD in his history of the Royal Society agrees with this. (22) The schedule includes the following work by BIRD :

		£.	s.	d.
«1746 Feb. 11	By an apparatus for trying the line of collimation, by Mr. BIRD	2	12	6
—— May 29	By an arch for the transit instrument, by Mr. BIRD	8	13	0
1747 July 9	By a level by Mr. BIRD	1	11	6
1748 Oct. 12	By alterations in the transit instrument, &c. by Mr. BIRD	5	15	6
1743-1748	....			
	By a brass mural quadrant (23) by Mr. BIRD	300	0	0
	By a moveable quadrant p^{r} ditto	200	0	0
	By a transit instrument ditto	73	13	6
	By a 20 feet refracting telescope p^{r} ditto	7	10	0
	By a barometer p^{r} ditto	2	12	6

(19) HORNSBY, THOMAS, editor, *Astronomical Observations, made at the Royal Observatory at Greenwich, from the year MDCCL. to the year MDCCLXII. by the Rev. James Bradley*, I, preface ii-iii (Oxford 1798).

(20) *Ibid.*, preface iii, and MASKELYNE, N., *op. cit.*, I, preface ii.

(21) 1750 may be considered as the year the new instruments were first used. The instruments were furnished in 1750 according to ABRAM ROBERTSON (editor), *Astronomical Observations, made at the Royal Observatory at Greenwich, from the year MDCCL. to the year MDCCLXII. by the Rev. James Bradley*, II, preface iii (Oxford 1805).

(22) WELD, CHARLES RICHARD, *History of the Royal Society*, I, 491 (London 1848); RIGAUD, S. P., *op. cit.*, lxxiii-lxxiv.

(23) BIRD, J., *The Method of Constructing Mural Quadrants*, 7 (London 1768). This part is quoted later in this article.

By a thermometer p[r] ditto	1 15 0
By alterations to the old mural quadrant p[r] ditto	32 10 0 » (24)

11. BIRD's brass quadrant was made for the west side of the wall, GRAHAM's being on the east side. (25) It is the one which BIRD described in 1768 in his book, *The Method of Constructing Mural Quadrants*, and to which we shall consequently return

AN

INTRODUCTION

AND

NOTES,

ON

MR. BIRD'S

METHOD OF DIVIDING

ASTRONOMICAL INSTRUMENTS.

TO WHICH IS ADDED, A

VOCABULARY

OF

ENGLISH AND FRENCH

TECHNICAL TERMS.

By W. LUDLAM,

LATE FELLOW OF ST. JOHN'S COLLEGE, CAMBRIDGE.

LONDON:

Sold by JOHN SEWELL, No. 32, Cornhill.

MDCCLXXXVI.

FIG. 1. The title page of Ludlam's *Notes*, a 4° of 42 pages. By courtesy of the library of Bowdoin College.

when discussing BIRD's two books. The two quadrants, BIRD's and GRAHAM's, continued in use for a considerable time, and in 1776 MASKELYNE wrote of them :

(24) RIGAUD, S. P., *op. cit.*, lxxiv.

(25) RIGAUD, S. P., *op. cit.*, lxxvii. [One description of the quadrant can be found in *Phil. Trans.* for 1806, II, 424 ff. It is also discussed in the memoirs of the French Academy for 1752 on pages 424 and 425.

« This [the division of 96] was first applied by Mr. Graham to the iron quadrant which he made for Dr. Halley, which was fixed up, on the east side of the pier with the telescope looking to the south, in the year 1725; but in the year 1753 it was taken down, and, after a new set of divisions had been applied to it by Mr. BIRD, was fixed upon the west side of the pier with the telescope looking to the north; the brass quadrant made by Mr. Bird, which had been placed for the three preceding years on the west side of the pier, for settling the latitude and refractions by the circumpolar stars, having been previously taken down and removed to the opposite or eastern side of the pier : and thus fixed both quadrants have continued ever since. » (26)

12. MASKELYNE also testified to the great accuracy of the brass quadrant, saying that its plane was so true and so well adjusted to the meridian that the stars passed its vertical wire within a few seconds of the true meridian transit as observed with the transit instrument, the greatest difference not exceeding 6 seconds of time. (27)

13. HORNSBY in his preface to BRADLEY's observations gives us more data concerning the quadrant, taking the date of its suspension, February 16, 1750, and its fitness for use in June, from BIRD's statement in the *Method of Constructing Mural Quadrants*. He then continues by saying that two loose sheets of paper show that several stars were observed at first with only the 90° arch, but that no observations were entered in the journal before the 10th of August, and that from then to the 24th of November the instruments were used without the telescope having been balanced. November 24th a counterpoise was applied, rendered necessary by the fact that the weight of the telescope upon the cylinder at the center, a variable quantity, was sufficient to affect the position of the instrument and to necessitate a constant rectification of the plumb-line after every observation. (28)

14. BIRD did not limit his work at Greenwich to the manufacture of new instruments, but also intelligently repaired the old. He admired the general construction of the old quadrant, and considered its greatest defect to be the considerable use of iron, which could not be forged as well as brass and thus contributed greatly to the alteration of the figure of the instrument. (29) He

(26) MASKELYNE, N., *op. cit.*, I, preface vii.
(27) *Ibid.*, I, preface viii.
(28) HORNSBY, T. (editor), *op. cit.*, I, preface vii.
(29) RIGAUD, S. P., *op. cit.*, lxxvii.

had contrived a small arc by which in 1750 and some of the preceding years the error of the line of collimation of the old quadrant had been measured and determined, by a mean of several trials, to be -6″. 0 by which the zenith distances were given too large, comparing with the zenith sector -3″. 9, from January 1751 to August 1752, -2″. 9, and from September 1752 to July 1753, -2″. 3. After the quadrant was directed to the north the zenith distances had to be diminished by 8″. 4. (30) This was after BIRD had furnished it with a new set of divisions, and it had been placed on the western side of the pier, (31) which, as we have seen, happened in 1753.

15. In 1787 (February 22) MASKELYNE read a paper before the Royal Society concerning the latitude and longitude of the Royal Observatory, which gave him occasion to speak again of the brass quadrant, and in glowing terms. He said that for over three years BRADLEY observed the pole star and other stars north of the zenith. After the positions of the old and the new quadrants had been interchanged, BRADLEY started the series of star and planet observations which was continued for so long. (32) MASKELYNE said that to confirm further confidence in the astronomical refractions, and the latitude of the observatory as determined by BRADLEY, an examination of the total arc of BIRD's quadrant had been made, from which BRADLEY found it to be accurate, differing at one time only a fraction of a second from 90° and at another time being perfect. MASKELYNE added that GRAHAM's quadrant had been similarly examined and found to be 16″ less than a quadrant, but that after BIRD redivided it in 1753 it was probably as accurate as the other. BRADLEY used the new divisions to redetermine the old, and to this effect MASKELYNE gave the following quotation from BRADLEY :

" « August 12, 1753, I measured with the screw of my micrometer the difference of the arcs (of $\frac{64}{96}$) as set off by Mr. GRAHAM originally, and by Mr. BIRD when he put on a new set of divisions upon the old quadrant, and I have found that Mr. GRAHAM's arc was less than Mr. BIRD's by $\frac{8}{40}$ divisions of my micrometer,

(30) HORNSBY, T., (editor), *op. cit.*, I, preface vii.
(31) *Ibid.*, I, preface xiv.
(32) *Phil. Trans.*, LXXVII, 154.

which to a radius of 96 inches answers to 10", 6; so that the whole arc of 96 differs from a true quadrant 15", 9, which is the same difference that I formerly found by means of the level, &c.» ' (33)

16. In the new building a short passage connected the room in which the quadrants were fixed with that intended for the transit. (34) This was a skillfully constructed instrument of BIRD's fabrication. POUND's and HALLEY's, among the first such instruments made, were probably supported by the walls, but the one which BIRD made for BRADLEY was supported by strong stone pillars. The axis of the instrument was originally intended to be four feet long, but was made to be four and a half feet. The telescope was 8 feet long. A counterpoise was applied to the ends of the axis to diminish the effect of friction on them by taking off part of the instrument's weight. An elliptical screen for illuminating the wires diminished the size of the aperture from 2.7 inches in diameter to 1.5. At first there was a single eyeglass which rendered the field of vision indistinct toward the edges, but in January 1753 a double eyeglass was applied as a remedy. The lenses were fixed, and when MASKELYNE entered into office he had BIRD make the nearest glass draw out so that the distance from it to the wires could be accommodated to the eye of each observer. In August 1772, only, did MASKELYNE apply a sliding eye piece to the telescope thus both avoiding the oblique view of the side wires and removing the occasion for the compound eyeglass. (35) The magnifying power of the instrument was 50 times the original. Five wires, as nearly as possible parallel to each other, were placed in the common focus of the eyeglass and the 8-foot focal-length object glass. The middle wire passed through the center of the telescope and was crossed by a sixth at right angles. Unfortunately, star observations were not made at each of the wires but only at the middle one. Such observations would have been useful in giving the time of passage over the central wire if not observed provided the distances between the wires were accurately known. BIRD had aimed to make these distances equal and by observation they

(33) *Ibid.*, LXXVII, 159-160.
(34) RIGAUD, S. P., *op. cit.*, lxxviii.
(35) *Idem.* See also MASKELYNE, N., *op. cit.*, I, preface iv.

were found to be nearly so. (36) Although the diameter of the tube was adapted to a common object glass of aperture less than 1.6 inches, the weight of the 8-foot transit instrument was so great that it was considered necessary to make provision against any alteration which the cylindrical ends of the axis might produce in the form of the metalline notches in which they moved. To do this, a lever was placed on each stone pier and applied to each end of the axis, and the counterpoise moved to adjust the weight on the axis. (37) According to BRADLEY the weight of the instrument was 55 pounds. January 4, 1755, by the above described arrangement, he placed the weights supporting it so that the pressure of each cylinder on the brass notch was three pounds. (38) According to MASKELYNE, the collimation error of this transit instrument was seldom more than 1″ and never more than 2″ except once in 1768. (39)

17. BRADLEY's schedule of expenditure included minor apparatus as well as the large quadrant and transit. BIRD made him a Fahrenheit thermometer which was sent to Paris to be kept for a time in the cave of the Royal Observatory, and returned to Greenwich with a mark on the scale about half a degree above the 50th division. Fifty degrees was considered the mean height of the thermometer in tabulating mean refraction. (40) BIRD also made a moveable quadrant, originally intended to be 4 feet in radius, but made 40 inches instead because of the inconvenience of the larger size. (41)

18. When the Imperial Academy of Sciences at St. Petersbourg refitted its observatory in the middle of the eighteenth century, the services of BIRD were enlisted. April 2, 1751 the astronomer A. N. GRISCHOW wrote to BRADLEY, asking him to supervise the work, although he realized BIRD was a skilled workman. (42) He explained that the observatory wanted to have an 8-foot mural quadrant made at London by BIRD, like the one in Green-

(36) HORNSBY, T., (editor), *op. cit.*, I, preface iii.
(37) *Ibid.*, I, preface iv.
(38) RIGAUD, S. P., *op. cit.*, lxxviii, note i.
(39) MASKELYNE, N., *op. cit.*, I, preface v.
(40) HORNSBY, T., (editor), *op. cit.*, I, preface xv.
(41) RIGAUD, S. P., *op. cit.*, lxxvii.
(42) *Ibid.*, 466-469, a letter from GRISCHOW to BRADLEY.

wich. The telescope was to roll on four little wheels and the frame was to have a double counterpoise like the mural belonging to the Earl of MACCLESFIELD. The frame was to be of a strong copper alloy. The divisions on the limb were to be of 90° and 96. By means of the nonius division and the little micrometer it would be possible to calculate the seconds. The limb was to include about 100°. He proposed a method for dividing the arc, and suggested that there be two plumb-lines. BIRD was to have the instrument ready to ship on the last boat leaving London for St. Petersbourg in 1751, since the observatory wished to make some observations corresponding to those of LACAILLE at the Cape of Good Hope (concerning the parallax of the moon).

19. I have found no statement to the effect that BIRD ever made such an instrument, but it is highly probable that he did. In the first place, GRISCHOW made observations for which he needed such a mural quadrant (43), and in 1750 he refers to his instrument as being of English make, saying :

« Zu diesem ende (nehmlich eine oder 2 gantze perioden vom monde zu observiren) haben besonders mein grosses englisches instrument und die exactitude, mit welcher es verfertiget worden, beigetragen, dass ich im stande bin die ascensionem rectam exactissime zu observiren, welches mit einem quadrantem Murali niemals so genau geschehen kann, weil man noch keinen einzigen verfertiget hat, wovon der limbus ein accurates planum ist. » (44)

At the end of the letter he speaks of the « grosser quadrant » and gives its cost as 600 Reichsthaler. (45)

20. Another instrument by BIRD was the 6-foot mural quadrant for TOBIAS MAYER of the University of Göttingen, (46) who was making solar and lunar observations. In 1754 MAYER was made director of the Göttingen observatory and King GEORGE II presented him with the instrument. (47)

21. Among the smaller instruments made by BIRD were several

(43) See *Methodus investigandi parallaxin Lunae et Planetarum eclipsibus stellarum fixarum a luna et planetis innixa. Auct. A. N. Grischow* in *Novi Commentarii Academiae Scientiarum Imperialis Petropolitanae*, IV, 451-474 (St. Petersbourg 1758).

(44) Letter by A. N. GRISCHOW in *Materials for the History of the Imperial Academy at St. Petersbourg for* 1750 (title in Russian), 698.

(45) *Ibid.*, 701.

(46) *Phil. Trans.*, LXXVII, 176-177.

(47) *Nouvelle Biographie Générale*, XXXIV, 538. For TOBIAS MAYER (1723-1762) see the *Allgemeine Deutsche Biographie*.

HADLEY's quadrants, (48) and HADLEY's sextants. (49) MASKELYNE speaks of a 6-foot sector by BIRD, which was used in settling the boundary between Maryland and Pennsylvania. (50) As we have seen above, this boundary was measured by MASON and DIXON, (51) who later measured the length of a degree, using the same instrument. (52) The sector was particularly accurate, having an altered suspension of the plumb-line which passed over and bisected a point at the center of the instrument. (53) According to MASKELYNE, BIRD made a similar alteration in the GRAHAM zenith-sector, substituting a fine piece of gold for the notch. (54) MASON and DIXON also used an instrument by BIRD which was serviceable as either an equal-altitude or a transit instrument, (55) and they had a 5-foot brass standard which on its return to London was compared with BIRD's scale, and reduced to the French measure. (56) MASKELYNE describes the method used by BIRD in finding the length of his scale in toises. (57)

22. BIRD's scale was, no doubt, the one made for the committees of the House of Commons in 1758 and 1759. These committees were desirous of ascertaining the original standards of weights

(48) *Tabulae motuum solis et Lunae novae et correctae; auctore Tobia Mayer: quibus accedit Methodus Longitudinum Promota eodem auctore.* [edited by MASKELYNE], CXXVII « *Extract of a Letter from Mr.* BENJAMIN ROBINS, *Engineer to the East-India Company, to Mr.* JOHN BIRD, *Mathematical-Instrument-maker, dated* Fort St. David's, Sept. 24, 1750. » (London 1770); also see, *Gent. Mag.*, XXVIII, 254 (June 1758).

(49) *Ibid.*, CXIII, CXIV and CXVI.

(50) MASKELYNE, N., *op. cit.*, I, preface X.

(51) *Phil. Trans.*, LVIII, 270; also *D. N. B.*, XXXVI, 417 (article *Mason, Charles*). For an account of the measurements see *Historical Magazine*, V, 199-202 (New York, July 1861).

(52) *Phil. Trans.*, LVIII, 270 ff. « *Introduction to the following Observations, made by Messieurs* CHARLES MASON *and* JEREMIAH DIXON, *for determining the Length of a Degree of Latitude, in the Provinces of* Maryland *and* Pennsylvania, *in* North America; *by the Reverend* NEVIL MASKELYNE, *B. D. F. R. S. Astronomer Royal.* » Read Nov. 24, 1768.

(53) *Idem.*

(54) MASKELYNE, N., *op. cit.*, I, preface X.

(55) *Phil. Trans.*, LVIII, 274 ff. « *Observations for determining the Length of a Degree of Latitude in the Provinces of* Maryland *and* Pennsylvania, *in* North America, *by Messieurs* CHARLES MASON *and* JEREMIAH DIXON. » A description of the transit instrument is given on page 274.

(56) *Phil. Trans.*, LVIII, 324-328.

(57) *Ibid.*, LVIII, 326-327.

and measures, and were greatly aided by BIRD. For the 1758 committee, in order to determine the capacity measures, he made an apparatus, under the direction of HARRIS, His Majesty's Assay Master of the Mint, by which the standard measures were placed on a plane leveled by four adjusting screws. Four brass cubes containing 300 cubic inches, sixty, twelve, and two, respectively, were also placed on brass planes and filled with water and made level by brass screws. (58) BIRD also furnished the committee with two brass rods made pursuant to the suggestions of HARRIS, who believed that a lineal measure ought to be the standard of all measures of capacity. The committee report gives HARRIS' ideas as follows :

« ... and as the Law seemed already to have made the Yard the Standard, he thought that to prevent Variations, it should be a clean strait Brass Rod, of about Thirty-eight, or Thirty-nine Inches long, and about an Inch broad and thick; near each End of this Rod, a fine Point should be made, and a fine Line drawn through it at right Angles to the Sides of the Rod; the Distance between the said two Points to be the true Standard Length of a Yard, and as the Edges of Points made in Brass were liable to decay, he would recommend two Gold Studs or Pins to be fixed in the Brass Rods, on which the Points were to be made, which ascertain the Length of the Yard. This Rod should be fitted in a proper Box and placed in safe Custody, to be used occasionally; but that for the ordinary sizing of Yards, there should be another Rod made in the same Manner with the former, only it should have two upright fixed Cheeks; by these Cheeks, anything placed between them might be more readily and exactly measured than by the Standards now at the Exchequer;... » (59)

Having a fixed standard yard the legislature could declare what proportion of it should be an inch, foot etc. To compare BIRD's two rods, the rod at the Exchequer, and GRAHAM's rod made for the Royal Society in 1742, the committee used beam compasses supplied by BIRD. (60)

23. BAILY in a report to the Royal Astronomical Society in 1835 speaks not only of the two rods by BIRD determining the standard yard of 1758 but also of a copy made in 1760 by the

(58) *Reports from Committees of the House of Commons, which have been printed by Order of the House,* and are not inserted in the Journals. Reprinted by Order of the House, II, 433. The report of the committee of 1759 is in the same volume.

(59) *Ibid.*, II, 434.

(60) *Idem.*

same artist, (61) both of which were destroyed in the fire in the Houses of Parliament in October 1834. (62)

24. The best sources of information concerning the mechanical part of BIRD's work are his own two books, *The Method of Dividing Astronomical Instruments* (1767) and *The Method of Constructing*

THE

METHOD

OF

Dividing ASTRONOMICAL INSTRUMENTS.

By Mr. JOHN BIRD,

MATHEMATICAL INSTRUMENT-MAKER, in the STRAND.

Published by ORDER of the

COMMISSIONERS of LONGITUDE.

LONDON,

Sold by JOHN NOURSE, in the Strand; and Meff. MOUNT and PAGE, Tower-Hill.

MDCCLXVII.

FIG. 2. The title page of BIRD's tract on the division of astronomical instruments, a 2° of VI plus 14 pages. By courtesy of the New York Public Library.

Mural Quadrants (1768). These attracted attention both in England and on the Continent, but some of the comment was derogatory. PIAZZI, the Italian astronomer, in a letter to LALANDE prefixed to LALANDE's French translation of RAMSDEN's (63) *Description of an engine for dividing mathematical instruments* (64) wrote :

(61) *Memoirs of the Royal Astronomical Society*, IX, 43-49, *Report on the Standard Scale of this Society* (London 1836).

(62) *Ibid.*, 79-82.

(63) JESSE RAMSDEN, 18th century English mechanician. See *D. N. B.*, XLVII, 265-267.

(64) Published by order of the Commissioners of Longitude (London 1777), 4°, 14 pp. The French translation is *Description d'une machine pour diviser*

« Graham et Bird se servoient du compas à verge ; celui-ci faisoit un mystere de sa méthode. Avant que le bureau des longitudes l'eût achetée pour la publier, M. Ramsden s'étoit fait déja une méthode comme Bird, et il l'avoit surpassé pour l'exactitude : ... » (65)

PIERRE-CHARLES LE MONNIER, (66) the French astronomer, was not gracious, either, concerning BIRD's books, although he recognized their value. He wrote as follows :

« La Commission nommée pour perfectionner la science des Longitudes, fit distribuer à Londres une grosse somme qu'elle avoit refusée à l'Auteur de l'Horloge Marine, pour qu'on décrivît avec soin l'Art de construire les Quarts de cercles muraux, comme aussi l'Art de les diviser : l'Artiste qui a reçu cette somme s'est contenté de produire un Ecrit avec trois planches gravées pour ce qui concerne la fabrication de l'Instrument ; & dès l'an 1767, un autre Ecrit concernant la division, avec une seule planche ou figure relative à son discours. On a cru devoir détailler ici davantage cette construction, en y joignant d'autres équipages très-utiles & leurs accessoires: le nombre des planches s'est accru nécessairement jusqu'au triple & au quadruple, ainsi que l'exigeoient les principaux détails où il a fallu entrer, & le Public loin d'être prévenu contre de pareils détails, en saura d'autant plus de gré à la Nation Françoise, qu'elle a su gratuitement les porter aussi loin que la chose sembloit le requérir. » (67)

LE MONNIER also disapproved of the apparatus for measuring the length of the whole arc, writing,

« La planche de sapin ou regle de 7 pieds & demi [Bird writes ' eight feet six inches '], avec deux arcs solides tracés à l'équerre, ou avec les arcs circulaires qui la terminent, qu'on a décrite à la *page* 24 de la *Méthode de construire les Quarts de cercles*, publiée à Londres en 1768, ne doit pas trouver place ici, étant d'une exécution trop difficile : cette méthode est d'ailleurs moins simple que celle de M. Graham, qui y employoit pareillement le niveau à bulle d'air, pour vérifier si l'axe répond à 90 degrés : ce célébre [sic] Méchanicien vouloit qu'on fît usage indépendamment de la lunette de l'alidade. » (68)

Explaining the construction of the frame-work of a quadrant,

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les instruments de mathematiques,... Traduite de l'anglois ; ... Par M. de la Lande, ... (Paris 1790), 4°, 46 pp., Plates.

(65) RAMSDEN, J., *Description d'une machine ...*, 6.

(66) *Nouvelle Biographie Generale*, XXX, 621-622.

(67) LE MONNIER, [PIERRE-CHARLES], *Description et Usage des Principaux Instruments d'Astronomie, Ou l'on traite de leur Stabilité, de leur Fabrique, et de l'Art de les Diviser*, 2, (1774) in Institut de France, *Description des Arts et Metiers*. This article has excellent plates. The maker of the « Horloge Marine » mentioned in this quotation is, probably, JOHN HARRISON.

(68) *Ibid.*, 4.

LE MONNIER preferred to refer to BIRD's book, describing his quadrant for the Greenwich Observatory, since GRAHAM's quadrant had not been forged from a single kind of metal. (69)

25. LUDLAM's *Notes* were written to make BIRD's pamphlet more clear, and contained the following none too gracious « attestation » by BIRD :

« When I wrote my Tract on the constructing and dividing of Astronomical Instruments, I thought I expressed myself so as to be understood by any common workman, a little used to this Business. What difficulties the *Learned* may find in it, I know not; if my friend can satisfy their scruples, so much the better. All I can say is that he has no where mistaken my meaning; and if he can (by what he has written) make those understand my method of dividing, who otherwise would not, he will not lose his labour.

JOHN BIRD.

June, 1773. » (70)

26. LUDLAM wrote that BIRD did not think it necessary to explain his « *principles* » to « common artificers », but that not every « Professor » would be acquainted with them. (71)

27. In 1767 the Commissioners of Longitude had offered BIRD a reward provided he would publish an account of his method of dividing astronomical instruments, and would demonstrate the process to persons chosen by the board, LUDLAM being among this number. In 1767, BIRD accordingly divided a 40-inch quadrant for the College of the Admiralty at Petersburgh before these people. LUDLAM also watched the dividing of an 18-inch quadrant in 1771 and took notes of what he thought was lacking in BIRD's tract. (72) Although LUDLAM referred to RAMSDEN's machine for dividing instruments he believed it still valuable to know the methods used for the Greenwich and Oxford (72a) instruments. He acknowledged his indebtedness to ALEX. AUBERT, Esq; F.R.S. & S.A. at whose expense his notes were published. (73)

28. Although LUDLAM did not think BIRD's tract sufficiently full in some details, he nevertheless appreciated all the refinements which BIRD had introduced into the process, and consequently

(69) *Ibid.*, 8.
(70) LUDLAM, W., *op. cit.*, v.
(71) *Ibid.*, preface iii.
(72) *Ibid.*, preface iv.
(72a) BIRD furnished the Radcliffe Observatory at Oxford.
(73) *Ibid.*, preface iv-v.

emphasized those difficulties which BIRD had satisfactorily met. For example, he wrote,

« The equality of the visible divisions will, therefore, depend partly upon the equality of the distances of the original points, and partly on the method of transferring the divisions from those. » (74)

And then he told how BIRD took care of this, writing,

« The equality of the original points will undoubtedly be affected by such partial expansions of the metal of which the instrument is made, as may take place while those points are setting out. Mr. Bird's method of guarding against this is, by setting out, first of all, a few capital points, distributed almost equally through the whole limb, leaving the intervals to be filled up afterwards; and thus shortening the time of the capital operations. Secondly, by taking care that both the scale of equal parts, and the quadrant to be divided from it, should be of the same temperature throughout, of course of the same degree of expansion; all of which is so clearly described by Mr. Bird, that it wants no comment. » (75)

29. BIRD worked with a beam compass. (76) He lay down his original points, following GRAHAM's principle of the practicability of bisecting an arch or right line but not trisecting, quinquesecting, etc. (77). Nor did BIRD approve of « stepping » or turning over the compasses, although he himself used that method in constructing his scale of equal parts. It was probably perfectly safe to step the compasses a few times, but not too many. (78) The arch of 96 which was introduced by GRAHAM and used by BIRD was based on the principle of bisection. (79)

30. By GRAHAM's method, the arch bounding the visible divisions was at some distance from the primitive circle, which was also the tangential circle. By BIRD's method, the bounding circle and primitive circle were very near and the tangential circle at some distance, because the radius with which the visible divisions were cut was long. (80)

31. In BIRD's method an alteration of the points of the compass was not serious. By GRAHAM's method, if the heat of the hand

(74) *Ibid.*, introduction 1
(75) *Idem.*
(76) *Idem.*
(77) *Ibid.*, introduction 2.
(78) *Ibid.*, introduction 3-4 and note.
(79) *Ibid.*, introduction 4.
(80) *Ibid.*, introduction 7.

should expand the beam compass in cutting divisions, the error of the expansion would be entirely repeated in the visible divisions, and similarly, if for any reason the distance of the points was changed; the expansion of the instrument would also occasion error. (81)

32. An expansion of the beam compass while cutting either nonius would make that nonius unequal, and so BIRD recommended holding the compass in the hand for a little while before cutting in order than it be of uniform temperature. (82)

33. LUDLAM gives the following excellent description of BIRD's scale of equal parts with its nonius :

« It contains, first of all, a scale of inches, each inch divided into tenths, and numbered at every inch from the left to the right, thus, 0. 1. 2. 3. &c. in the order of the natural numbers. The nonius scale is below this, but contiguous to it; so that one common line terminates the bottoms of the divisions on the scale of inches, and the tops of the divisions on the nonius; this nonius scale contains, in length, 101 tenths of an inch; this length is divided into 100 equal parts, or visible divisions; the left hand end of this scale is set off, from a point $\frac{1}{10}$ of an inch to the left of 0 on the scale of inches; therefore, the right hand end of the scale reaches to and coincides with the 10th inch on the scale of inches. Every tenth division on this nonius scale is figured from the right to the left, thus, 100. 90. 80. 70. 60. 50. 40. 30. 20. 10. 0. and thus 0 on the nonius coincides with 10 on the inches; and 100 on the nonius falls against the first subdivision (of tenths) to the left hand of 0 on the inches; and these two, viz. the first and last, are the only two strokes that do coincide in the two scales. » (83)

LUDLAM continues by saying that when the nature of the scale is known, it will not be difficult to understand the directions for dividing it, and that the scale is preferable to any diagonal scale because the latter has no check upon the errors. He adds,

« ... here the uniform manner in which the strokes of one scale separate from those of the other, is some evidence of the truth of both; but Mr. Bird's method of assuming a much longer line than what is absolutely necessary for the scale, subdividing the whole by a continual bisection and pointing the divisions as before explained, and guarding against partial expansions of the metal, is sure to render the divisions perfectly equal. The want of such a scale of equal parts (owing, perhaps, to their ignorance of constructing it) is one reason why Mr. Bird's method of dividing is not in so great estimation, among mathematical instrument makers, as it justly deserves. » (84)

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(81) *Ibid.*, introduction 8.
(82) *Ibid.*, notes 17.
(83) *Ibid.*, notes 21-22.
(84) *Ibid.*, notes 23-24.

34. According to LUDLAM, BIRD's first intention was to write on the construction of a quadrant before explaining how to divide it, but he was ordered to do otherwise (85). Thus his first tract, the one discussed by LUDLAM, is on the division of astronomical instruments. It is a folio of VI+14 pages with a preface by MASKELYNE and one by BIRD. It was published by order of the Commissioners of Longitude and printed in London in 1767. MASKELYNE in his preface gives the conditions laid down for BIRD by the Commissioners of Longitude, which were as follows: he was to receive £ 500 upon taking an apprentice for seven years and teaching him his art and method of making astronomical instruments and also instructing persons directed by the Commissioners from time to time; and also delivering in writing, on oath, to the Commissioners, a complete description of his method of constructing and dividing astronomical instruments, particularly like those he made for the Greenwich Observatory, accompanied by minute exact plates of those instruments for which he was to be allowed the further sum of £ 60. According to MASKELYNE, on March 21, 1767 BIRD delivered a plate and the tract on dividing the mural arc at Greenwich and on the improvements he had made meanwhile, whereupon the Commissioners published that account, intending that the part on constructing astronomical instruments should follow it. (86)

35. In his own preface BIRD said that his method was collected from 34 years of experience and from the teaching of SISSON. (87) He stressed the importance of accurate instruments in the following words:

> « How far the Lunar Theory hath been improved by the observations of the late Dr. Bradley, and Mr. Mayer, I leave to the decision of those who have tried it by observations, in order to find the Longitude at sea, &c. I cannot help, however, being fully of opinion, that a still more perfect knowledge of the motion of the heavenly bodies may be obtained by future observations, skilfully made, with accurate Instruments. » (88)

36. BIRD's version of the origination of the book was this:

(85) *Ibid.*, introduction 9.
(86) BIRD, J., *The Method of Dividing Astronomical Instruments*, preface iii-iv (London 1767).
(87) *Ibid.*, preface v.
(88) *Ibid.*, preface v-vi.

« At a Board of Longitude, held at the Admiralty on the 10th of January, 1767, it was required of me to describe my method of dividing Astronomical Instruments, and the said description to be delivered at a subsequent Board, to be held on the 14th of March following. » (89)

37. He gave the following description of his tools :

« *The requisites for the performance of this work are as follow. A scale of equal parts, by which the Radius may be measured to* 0,001 *of an inch, must be provided. My scale is* 90 *inches long, each inch divided into* 10, *contiguous to which are nonius divisions, viz.* 10,1 *inches divided into* 100 *equal parts, shewing* 0,001 *of an inch; and by the assistance of a magnifying glass, of one inch focal length, a third of* 0,001 *may be taken off by estimation.*

Provide five Beam Compasses, to which magnifying glasses, of not more than one inch focal length, should be applied. Let the longest beam be sufficient to draw the Arcs, and measure the Radius : The 2d *to measure the Chord of* 42°:40' : *The* 3d *to measure the Chord of* 30° : *The* 4th 10°:20' : *The* 5th 4°:40' : *And if a* 6th, *to measure* 15°, *be made use of, so much the better....* » (90)

38. The care with which BIRD worked is well illustrated by his own description of the 8-foot quadrant for the Royal Observatory, which reads thus :

« *The Radius of the Arc of* 90°, *at the points,* = 95,938 *inches, from which the following numbers were computed, viz.* 49,6615 *inches = Chord of* 30° — 25,0448 *inches = Chord of* 15° — 17,679047 *inches = Chord of* 10°:20' — 7,81186 *inches = Chord of* 4°:40' *and* 69,80318 *inches = Chord of* 42°:40' *Having drawn the several Arcs, between which the divisions were to be cut, the Radius and the lengths of the above Chords were taken by the beam Compasses, which, together with the Scale, were laid upon the Quadrant, where they remained till the next morning; during which time the door of the room was kept locked. Before the sun-rise I measured the Radius, which required some correction; the Beam being of white fir, and the Scale of brass, which probably contracted, while the Beam remained unaltered : The other Beam Compasses also required correction.* » (91)

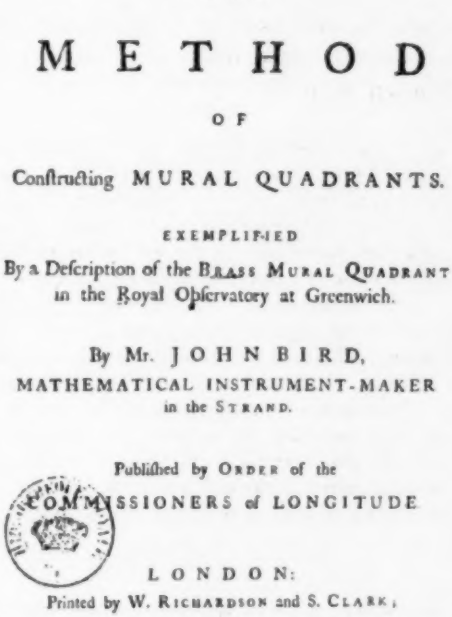
39. BIRD said that his arc of 96 was divided into three equal parts, like the 90° arc, and that each third contained 512 divisions, which gave 16 in each 96th part of the whole arc. (92)

40. Having learned by experience that the greatest difficulty in laying off the principal points, especially when this took several hours, was caused by the expansion or contraction of the instrument

(89) *Ibid.*, 1.
(90) *Ibid.*, 2.
(91) *Ibid.*, 2-3.
(92) *Ibid.*, 5.

to be divided, occasioned by the increasing or decreasing heat of the sun, BIRD contrived to lay them off in the least possible time before any expansion or contraction could take place. Three or four persons in the room might have the same effect as the heat of the sun, so BIRD only admitted one as an assistant. Nor did he allow a fire in the room until the principal points were laid. By having all the chords computed and measured the evening before they were to be laid off, he could in a few minutes accomplish what by trial would require some hours. To exercise further caution, he advised that the principal points be laid off before sunrise or on a cloudy morning. (93)

41. *The Method of Constructing Mural Quadrants* was published the year after the treatise on dividing instruments, namely, in



THE

METHOD

OF

Conſtructing MURAL QUADRANTS.

EXEMPLIFIED

By a Deſcription of the BRASS MURAL QUADRANT in the Royal Obſervatory at Greenwich.

By Mr. JOHN BIRD,
MATHEMATICAL INSTRUMENT-MAKER
in the STRAND.

Publiſhed by ORDER of the
COMMISSIONERS of LONGITUDE

LONDON:
Printed by W. RICHARDSON and S. CLARK;
AND
Sold by JOHN NOURSE, in the Strand; and Meſſ. MOUNT and PAGE, Tower-Hill.
M DCC LXVIII.

FIG. 3. The title page of BIRD's tract on constructing mural quadrants, a 4° of 27 pages. By courtesy of the Bibliothèque Nationale, Paris.

(93) *Ibid.*, 10-11.

1768. It has one preface, written by MASKELYNE, and identical word for word with the preface by the same astronomer to the first tract except for the substitution in place of the last paragraph of a statement that the first tract had been published, and the addition of the following paragraph :

« Mr. Bird, in further pursuance of his engagements, having since delivered to the Commissioners three other plates, intituled N° 1, 2, 3, with a written paper, containing an explanation of the said plates, and an account of his method of constructing Mural Quadrants, exemplified by a description of the Brass Mural Quadrant in the Royal Observatory, the Commissioners have ordered this also to be published, which forms one work with the Method of Dividing, published before. An exact Model of the Brass Mural Arc in the Royal Observatory, to half the size, has been also provided by Mr. Bird, and is now deposited, by order of the Commissioners of Longitude, in the British-Musaeum. » (94)

42. In the first paragraph of the book BIRD tells how he started to work at Greenwich, and how he came to the conslusion that brass should be used in place of iron when forging quadrants. He expresses himself thus :

« In the year 1748, I was informed by the late Dr. Bradley, that application had been made for a new Mural Quadrant, to be fixed to the west side of the pier in the Royal Observatory, in order take [sic] observations to the North; that he had great hopes of success, and therefore desired that I would consider how to prevent a fault which he had found in the old Quadrant; which was, that it had altered its figure by its own weight, so as to render the whole Arc 16" less than a Quadrant or 90°. Accordingly, I made myself fully acquainted with the general construction of the old Quadrant, which was executed under the direction of the late Mr. Graham, and found the general plan, though little taken notice of at that time, to be such, as, I think, will be a lasting testimony of his great skill in mechanics. The reason of the alteration by its own weight, seems to be a defect in the manner of fastening the several parts together, probably owing to the cocks, and plates for that purpose, being of iron, which could not be forged in that advantageous shape, which I afterwards contrived to give to those that were cast of brass, for the new Quadrant; an order for the making of which, I received in February 1749. » (95)

43. The plates were drawn with unusual care and in great detail. Plate I shows the brass structure of the quadrant. BIRD tells why GRAHAM used 6 cocks cemented into the pier, and

(94) BIRD, J., *The Method of Constructing Mural Quadrants*, preface vi (London 1768).

(95) *Ibid.*, 7-8.

why he himself did not do this in the instruments which he made after the Greenwich quadrant. His argument was as follows :

« It is next to impossible to make the arch of a Quadrant so true a plane, as not to draw, in some places, the Telescope out of the plane of the Meridian; though this does not sensibly affect the observations, with regard to the Altitude, or Zenith distance, Mr. Graham thought proper to provide a remedy by cementing six cocks into the pier; those cocks having each two screws, which point towards each other, and receive between them other cocks, screwed to the inside of the perpendicular arch : by easing one screw, and screwing up the other, the limb of the Quadrant may be pushed towards or from the face of the pier, as occasion requires, which may be easily known by a Transit Instrument placed in the Meridian; for the difference in the times of a Transit shewn by the Transit Instrument and the Quadrant shews the error of the latter.

It was the intention of Dr. Bradley to put the old Quadrant and the new, in each other's place, after two or three years observations to the North with the new one; I was therefore obliged to make the new Quadrant of the same radius with the old, and in every respect to fit its place. I have mentioned above, that the cocks to reduce the plane of the arch to the Meridian, are cemented into the pier : these afterwards gave me a great deal of trouble, that I was, at first, not aware of; for it was not without the utmost difficulty that the Quadrant could be hung upon the pins; which difficulty was wholly occasioned by these cocks being in the way : I therefore, in all the Mural Quadrants which I have since executed, made them in the following manner. Let *xy* represent the face of the pier, *pz* two sections of a plug made fast in the pier with melted lead, *vv* two sections of the cock screwed to that plug, and K a section of the arch and cock, which falls in between the screws above mentioned : .... These being fixed on the outside of the perpendicular arch (on the inside of the instruments in the Royal Observatory) require the plugs with their cocks on the outside also : they may be taken away and replaced at pleasure, by which means all interruption from them is prevented. » (96)

44. The second plate pictures the front of the quadrant in a vertical position.

45. Plate III represents the telescope of the instrument with all its apparatus and is described in ten parts, each part represented by a separate figure. Concerning the bracing of the telescope, BIRD wrote :

« The late Dr. Bradley, Astronomer Royal, at the time when I made this instrument, was very desirous of having the Telescope to perform its office without bracing; I therefore hammered the tube extremely hard, and made the trial, but without success; it was therefore braced, in imitation of the Telescope of the old Quadrant. » (97)

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(96) *Ibid.*, 10-11. The letters in the quotation refer to BIRD's plate.
(97) *Ibid.*, 15.

He believed a counterpoise necessary, and therefore wrote :

« The Telescope of a large Mural Quadrant would scarce be manageable without a counterpoise, to keep it at rest in all parts of the arch... This counterpoise performs two parts; it does not only balance the Telescope, but the crank also, which lies on one side of the Telescope when perpendicular to the horizon : this is done by putting the brass socket, through which the iron arm goes, above the middle of the lead, which may be cast three or four pounds heavier than is necessary, to balance the Telescope, and then may be cut away, from that end which requires it. » (98)

46. After the telescope was fastened to the quadrant, BRADLEY noticed that when the eye-end of the telescope was brought down to 0° the cylinder bore all the weight, so BIRD contrived a method by which in all positions of the telescope the cylinder at the center would never bear more or less than half its weight. (99) BIRD described how he placed the wires in the reticule. (100)

47. His mechanical description of the instruments is followed by a history of the Greenwich quadrant, which reads as follows :

« On the 16th of February 1750, the Quadrant above described was suspended upon the west side of the pier in the Royal Observatory, and in June following was ready for observation. In the beginning of the year 1753 Dr. Bradley acquainted me, that he was fully resolved to have the two Quadrants put in each other's place, in the following summer, being satisfied with the observations which he had taken to the north, with the new one; but before this movement was effected, it would be proper to try if the Quadrant retained its true figure, having been in a vertical position near three years. Accordingly an apparatus for that purpose was provided, and about the latter end of January, the same year, the trial was made by Dr. Bradley, who found the whole Arc, to be two seconds less than a Quadrant : I also tried it and found it to be one second and a half less than a Quadrant. From hence we concluded, that as the deviation from a true Quadrant was so extremely small, the error lay in our trials, rather than in the instrument. However the doctor still remained unsatisfied, and was desirous of knowing if the Telescope described the same arc that was shewn by the divisions : a very simple apparatus (101), which I shall describe, was prepared, and the trial made by Dr. Bradley, who found the Telescope, when set to the 90th degree, to shew the whole Arc, to be three quarters of a second greater than a Quadrant, and by my trial half a second, greater than a Quadrant. This astonishing agreement is scarce to be believed, but the truth of it will, I hope, be confirmed, whenever Dr. Bradley's papers are referred to inspection.

(98) *Ibid.*, 16.

(99) *Ibid.*, 18-19.

(100) *Ibid.*, 19-20.

(101) By this apparatus both the Telescope and Arc are at once examined. (BIRD's footnote on page 23.)

In July 1753 I took down the old Quadrant, put a new set of divisions upon it, and put the new Quadrant in the place of the old, and the old one in the place of the new.

On the 28th of July 1759 the new Quadrant was again examined by Dr. Bradley and myself, when we found the whole Arc to be exactly 90°. The last examination was in very hot weather, and the first in very cold; so that it is evident, that neither hot nor cold weather, nor the weight of the instrument, have any material effect upon its figure.» (102)

48. BIRD ends his paper with a rather extravagant statement in praise of his instrument and its general construction, saying that if properly used it would last for many ages without diminishing in value. (103) He closed the paper by stating that the most critical examination of an instrument occurs in the very process of dividing it, and he recommended relying on the arch of 96 divisions rather than the 90° arch when computing from observations taken with the Greenwich quadrant. (104)

49. He died March 31, 1776 at the age of 67, (105) probably in London.

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(102) BIRD, J., *Method of Constructing Mural Quadrants*, 23-24.

(103) *Ibid.*, 26.

(104) *Ibid.*, 27.

(105) *Gent. Mag.*, XLVI, 192, and *Additional M. S.* 5728 of the British Museum, a volume of MUSGRAVE's Obituary, by letter from H. IDRIS BELL, Keeper of the MSS.

INDEX OF PROPER NAMES

Numbers refer to paragraphs.

Aubert, Alex. Esq., F. R. S. & S. A.: 27.
Auckland, see Bishop Auckland.
Baily, Francis: 23.
Bird, John: passim.
Bishop Auckland, Durham County: 3.
Bliss, Nathaniel: 6.
Bradley, James: passim.
British-Museum: 41.
Cape of Good Hope: 18.
Cockfield, Durham County: 3.
College of the Admiralty at Petersburgh: 27.
Dixon, Jeremiah: 3, 21.
Durham County, see Bishop Auckland, Hurworth, and Cockfield.
Emerson, William: 3.
George II: 9, 10, 20.
Göttingen, University of: 20.
Graham, George: passim.
Greenwich, see Royal Observatory.
Grischow, Auguste-Nathanael: 18, 19.
Hadley, John: 21.
Halley, Edmund: 8, 11, 16.
Harris, Joseph: 22.
Harrison, John: 24.
Hornsby, Thomas: 6, 7, 13.
Hurworth, Durham County: 3.
Imperial Academy of Sciences at St. Petersbourg: 18, 19.
La Caille, Nicolas-Louis de: 18.
LaLande, Joseph Jérôme Le Français de: 24.
Le Monnier, Pierre-Charles: 24.
Ludlam, William: 3, 5, 25, 26, 27, 28, 33, 34.
Macclesfield, Earl of, see Parker, George.
Maryland, see Mason and Dixon Line.
Maskelyne, Nevil: 8, 11, 12, 15, 16, 21, 34, 41.
Mason and Dixon Line: 3, 21.
Mason, Charles: 3, 21.
Mayer, Tobias: 20, 35.
Paris, see Royal Observatory, Paris.
Parker, George, second Earl of Macclesfield: 6, 7, 18.
Pennsylvania, see Mason and Dixon Line.
Piazzi, Joseph: 24.
Pound, James: 16.
Ramsden, Jesse: 24, 27.
Rigaud, Stephen Peter: 5, 7, 10.
Royal Astronomical Society: 23.
Royal Observatory, Greenwich: passim.

Royal Observatory, Paris : 17.
Royal Society : 6, 7, 10, 15, 22.
St. Helena : 3.
St. Petersbourg, see Imperial Academy of Sciences at St. Petersbourg.
Shirburn Castle : 6, 7, 18.
Sisson : 4, 7, 35.
Weights and Measures, Committees on, in the House of Commons : 22, 23.
Weld, Charles Richard : 10.
Woolwich Academy : 3.

Discovery of conical refraction by William Rowan Hamilton and Humphrey Lloyd (1833)

With facsimile reproduction (no. XIV) of HUMPHREY LLOYD'S *first paper On the Phaenomena presented by Light in its Passage along the Axes of Biaxal Crystals, originally published in The London and Edinburgh Philosophical Magazine and Journal of Science, conducted by Sir* DAVID BREWSTER, RICHARD TAYLOR *and* RICHARD PHILLIPS *(vol. 2, 112-20, 1833).*

1. From 1827 to 1833, WILLIAM R. HAMILTON published in the *Transactions of the Royal Irish Academy* a series of papers wherein the wave theory of light, then recently elaborated by AUGUSTIN FRESNEL (1788-1827), was brilliantly developed. (1) The last of these papers entitled « Third supplement to an essay on the theory of systems of rays » was read at the Academy January, 23, 1832 and October 22, 1832. The most startling part of that great memoir, or to use HAMILTON's phrase « the least underserving of attention » read at the second of these meetings, was the mathematical prediction of conical refraction.

Perhaps the simplest way of explaining this is to borrow the words of a contemporary account in the *Dublin University Magazine* of January, 1842. As quoted by GRAVES (vol. 1, 623).

« The law of the reflexion of light at ordinary mirrors appears to have been known to EUCLID; that of ordinary refraction at a surface of water, glass, or other uncrystallized medium, was dis-

(1) The first of these papers, Theory of systems of rays, was read by HAMILTON on Dec. 3, 1824, but only published in vol. 15 of the *Transactions*, part 1, 1827. According to his own statement he introduced many modifications into it after 1824; the correct date of it is June 1827. The first and second supplements were published in parts 1 and 2, vol. 16, 1830-31; the third, in part 1 of vol. 17. summer 1833.

covered at a much later age by SNELLIUS; HUYGHENS discovered, and MALUS confirmed, the law of extraordinary refraction produced by uniaxal crystals, such as Iceland spar; and finally the law of the extraordinary double refraction at the faces of biaxal crystals, such as topaz or arragonite, was found in our own time by FRESNEL. But even in these cases of extraordinary or crystalline refraction, no more than *two* refracted rays had ever been observed or even suspected to exist, if we except a theory of CAUCHY, that there might possibly be a *third* ray, though probably imperceptible to our senses. Professor HAMILTON, however, in investigating by his general method the consequences of the law of FRESNEL, was led to conclude that there ought to be in certain cases, which he assigned, not merely two, nor three, nor any finite number, but an *infinite* number, or a *cone* of refracted rays *within* a biaxal crystal, corresponding to and resulting from a *single* incident ray; and that in certain other cases, a single ray within such a crystal should give rise to an infinite number of emergent rays, arranged in a certain other cone. He was led, therefore, to anticipate from theory two new laws of light, to which he gave the names of *Internal and External Conical Refraction.* »

2. HAMILTON was so sure of the physical reality of facts revealed to him by his equations, that he asked a colleague of his, the Rev. HUMPHREY LLOYD, to institute experiments for the purpose of verification. He made that request to LLOYD on October 23, that is, on the morrow of the communication of his mathematical discovery to the Irish Academy. LLOYD sat to work at once and after much labor obtained the first experimental confirmation on December 14. I need not explain his experiments as this is done in the paper reproduced in facsimile below, which appeared in the February number of the *Philosophical Magazine* (a very prompt publication indeed !) This paper dealt only with the external conical refraction, but soon afterward he was able to evidence also the internal refraction, and explained it in a second paper which appeared in the March number of the same magazine (p. 207-10). « Further experiments on the phaenomena presented by light in its passage along the axes of biaxal crystals ». I do not reproduce this second paper as it is less striking. When the physical existence of the external refraction has been proved, that of the internal one was almost taken for granted.

3. There is no doubt that the credit of that discovery belongs primarily to HAMILTON. It was indeed a great achievement, comparable to the mathematical discovery of Neptune by ADAMS and LEVERRIER (1846), and the most remarkable confirmation of the wave theory of light. Some sceptical men of science like to deny the absolute validity of scientific theories, and to consider them only as clever means of accounting for the facts, or as PLATO put it of « saving appearances » (*σώζειν τὰ φαινόμενα*). And indeed the continual readjustments of theories to facts—as was the case e.g. in Greek astronomy—give one such impression. A game in which the rules are changed according to our needs, may seem a little tricky. However such objections are untenable with regard to predictions like those made by HAMILTON, and a little later by ADAMS and LEVERRIER, for in such cases the facts follow the theories. Moreover these predictions are not vague like the ancient prophecies, but on the contrary extremely precise. The mathematician suggests experiments of which the physicist would never have thought, experiments of the utmost singularity, the circumstances of which are defined with the greatest accuracy. The experiments may be so complicated and difficult that the physicist is hard put to institute them, but he finally succeds, and the predictions of the mathematicians are fully realized,—quantitatively as well as qualitatively.

4. We now know better than was possible a century ago that a theory leading to such uncanny confirmations is not necessarily the complete and final truth, but we are sure that it must be a part of it. In particular we realize that the wave theory of light reveals but one side of reality, but side at least it reveals wonderfully well. In general, we realize that no scientific theory is absolutely true; it is only an approximation, yet we believe in the absolute validity of scientific theories in a different way. To know that one does not know the whole truth but that one is gradually approaching nearer to it, and to be able at almost every step to measure the approximation of one's knowledge, is a kind of absolute knowledge not actual but potential,—a knowledge which is certain but limited, real and complete but deferred.

5. To return to HAMILTON, it would have been more logical perhaps to publish a facsimile of his paper than of LLOYD's, but this was impossible because HAMILTON's Third supplement

is very long (144 p., quarto) and illegible except for advanced mathematicians. It is true the part dealing with conical refraction is relatively short, but it would not be expedient to detach it from the context. That part is entitled (p. 134-38) :

« New consequences of FRESNEL's principles. It follows from those principles, that crystals of sufficient biaxal energy ought to exhibit two kinds of conical refraction, an external and an internal : a cuspray giving an external cone of rays, and a normal of circular contact being connected with an internal cone. »

6. Morevoer, HUMPHREY LLOYD was not simply a clever experimentalist, he had a share in the elaboration of the theory. He it was who discovered that all the rays of the cone were polarized in different planes; and who formulated the law of conical polarization, namely : « the angle between the planes of polarization of any two rays of the cone is half the angle contained by the planes passing through the rays themselves and its axis » (see facsimile below, top of p. 117), and showed that that law was but another consequence of FRESNEL's principles. It is important to insist upon LLOYD's share in HAMILTON's work, because the law of polarization, reexplained by HAMILTON in his own language, is found in the latter's memoir (p. 138-41) supposedly read on Oct. 22, 1832, without reference to LLOYD. This shows once more how distrustful one must be of the date of presentation of a discovery.

The facts are as follows. HAMILTON read his Third supplement at two meetings of the Irish Academy, Jan. 13 and October 22, 1832. At the second of these meetings he made his prediction of the rare phaenomena of conical refraction, internal and external. Then LLOYD began his experiments, was soon able to evidence the physical reality of conical refraction; and discovered the law of conical polarization. He gave an account of his discovery at the Irish Academy on January 28, 1833. The number of the *Transactions* of that Academy containing HAMILTON's and LLOYD's memoirs, and no others (160 p.) appeared in the summer of 1833. (The volume of which that number was the first part, vol. XVII, was completed only in 1837, and bears that date). While it was printing HAMILTON added the chapter on conical polarization and an introduction, and made perhaps other changes.

Of course every scholar will be tempted to insert improvements in his MS. almost until the last minute. Hence it is the date of publication and not of presentation or reading which is the most important : that is why I gave in my title 1833 as the date of discovery, though I know that as far as HAMILTON was concerned it was anterior to October 22, 1832 and that LLOYD saw it with his own eyes in December 14 of the same year.

7. Another colleague of HAMILTON's in Dublin, JAMES MC CULLAGH (1809-47), had read at the Irish Academy, on June 21, 1830, a paper on « The double refraction of light in a crystallized medium according to the principles of FRESNEL » (*Transactions*, vol. 16, part 2, p. 65-78). When MCCULLAGH heard of the discovery of conical refraction he claimed that it was but « an obvious and immediate consequence » of the theorems published by him in 1830. This might have been the start of an ugly priority discussion, but for the perfect gentleness and honorability of the three protagonists, HAMILTON, LLOYD, and MCCULLAGH. It is very true that MCCULLAGH had been very close to making the discovery, and this is especially interesting because he used geometrical methods as opposed to the purely analytical methods of HAMILTON. Still a third mathematician had been on the path, GEORGE BIDDELL AIRY (1801-92), who wrote to HAMILTON that he had long been aware of the existence of conoidal cusps (or « dimples » as he once called them) in FRESNEL's wave. Indeed FRESNEL might have perceived them himself.

MCCULLAGH, AIRY, even FRESNEL, might have made the discovery; the fact remains that they failed while HAMILTON succeeded. HAMILTON was the real discoverer, and HUMPHREY LLOYD, the confirmer.

8. Contemporaries were not slow in realizing the importance of that discovery. I shall quote only a single witness, the great mathematician JULIUS PLÜCKER (1801-68), who stated at the conclusion of his « Discussion de la forme générale des ondes lumineuses » (*Crelle's Journal*, vol. 19, 1-44, 1839) (PLÜCKER's French was sometimes faulty; I quote him verbatim) :

« Aucune expérience de physique a fait tant d'impression sur mon esprit, que celle de la réfraction conique. Un rayon de lumière unique entrant dans un crystal et en sortant sous l'aspect d'un

cone (2) lumineux : c'étoit une chose inouie et sans aucune analogie. Mr. HAMILTON l'annonça, en partant de la forme de l'onde, qui avoit été déduite par des longs calculs d'une théorie abstraite. J'avoue que j'aurois désesperé de voir confirmé par l'expérience un résultat si extraordinaire, prédit par la seule théorie que le génie de FRESNEL avait nouvellement créée. Mais Mr. LLOYD ayant démontré que les expériences étoient en parfaite concordance avec les prédictions de Mr. HAMILTON, tout préjugé contre une théorie si merveilleusement soutenue, a dû disparaître. En même temps la forme générale, indiquée aux ondes lumineuses par FRESNEL a dû acquérir une importance toute particulière... »

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9. According to our wont we shall conclude this paper with some biographical data.

HAMILTON was born in Dublin, 1805; he was partly of Irish and partly of Scottish descent; he showed amazing precocity in the study of languages as well as mathematics, was educated in Trinity College, Dublin, and at the age of 22 appointed professor of astronomy and superintendent of the observatory. The rest of his life was spent at that observatory, located at Dunsink, near Dublin. Soon after 1827 he became astronomer royal for Ireland. In 1835 one of the Royal Medals of the Royal Society was awarded to him for his discovery of conical refraction, the other medal of that year being bestowed upon FARADAY. He was knighted in 1835, and died in 1865. Of course his fame is mainly based upon his discovery of the quaternions (Oct. 16, 1843, published 1844) and his astounding elaboration of them in the Lectures on quaternions (1853) and in Elements of quaternions, posthumously edited by his son (1866). Though the quaternions have fallen short of the expectations to which they had given rise and are already on the road to oblivion, they represented a creative effort of the very first order, and HAMILTON will always be counted one of the greatest mathematicians of the nineteenth century.

A very detailed biography of him was composed by ROBERT

(2) This ought to read cylindre.

PERCEVAL GRAVES : *Life of Sir William Rowan Hamilton* (3 vols., Dublin, 1882-1889). The discussion of conical refraction will be found mainly in vol. 1 (p. 623-38, 685-88). Of the many other references to it, the only worth mentioning occurs in vol. 3, p. 146. Somebody having remarked, « I know of no person who has not seen conical refraction that really believed in it. I have myself converted a score of mathematicians by showing them the cone of light », HAMILTON replied : « How different from me ! If I had seen it only, I should not have believed it. My eyes have too often deceived me. I believe it, because I have proved it. » Mathematicians will appreciate the humour of this. HAMILTON made a similar statement in a letter to AUGUSTUS DE MORGAN in 1852 (vol. 3, 350).

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10. Though he was not a giant like HAMILTON, HUMPHREY LLOYD was one of the most distinguished Irish scientists of his time. He was born in Dublin, 1800, was educated at Trinity College, and in 1831, succeeded his father as professor of natural and experimental philosophy in that college. His greatest achievement was the very one discussed in this paper : the experimental confirmation of conical refraction and the discovery of the law of conical polarization (1833). He wrote various treatises on optics, magnetism, and meteorology. He was one of the organizers of research in terrestrial magnetism in the British Empire. He was president of the Royal Irish Academy from 1846 to 1851, president of the Dublin meeting of the British Association in 1857, and provost of Trinity College from 1867 to his death in 1881. See notice by THOMAS HAMILTON in the *Dictionary of national biography* (vol. 33, 425-26, 1893). There are many references to him in GRAVES's Life of HAMILTON.

Cambridge, Mass. GEORGE SARTON.
August 18, 1930.

INDEX

The numbers 1 to 10 refer to SARTON's introduction, the numbers 112 to 120 to LLOYD's paper.

ADAMS, J. C. 3.
AIRY, G. B. 7.
HAMILTON, T. 10.
HAMILTON, W. R. 9, passim.

CAUCHY, 1.
DE MORGAN, A. 9.
DOLLOND. 114.
EUCLID. 1.
FARADAY. 9.
FRESNEL, A. 1, 5, 6, 7, 8, 112, 113.
GRAVES, R. P. 1, 9.
HADLEY, J. 117.
HUYGHENS, 1, 112.
LEVERRIER, 3.
LLOYD, H. 10, passim.
MALUS. 1.
MCCULLAGH, J. 7.
PLATO. 1.
PLÜCKER, J. 8.
RUDBERG, F. 114, 117, 120.
SNELLIUS. 1, 112.

The following facsimile is published thanks to the courtesy of the Library of Congress and of Mr. FREDERICK BRASCH. We owe the photograph of LLOYD's bust to the courtesy of Trinity College, Dublin, and of Mr. W. A. GOLIGHER, its registrar. There is also a painted portrait in Trinity College but according to Mr. GOLIGHER, the bust is decidedly better.

XXI. *On the Phænomena presented by Light in its Passage along the Axes of Biaxal Crystals. By the Rev.* HUMPHREY LLOYD, *A.M. M.R.I.A. Fellow of Trinity College, and Professor of Natural and Experimental Philosophy in the University of Dublin**

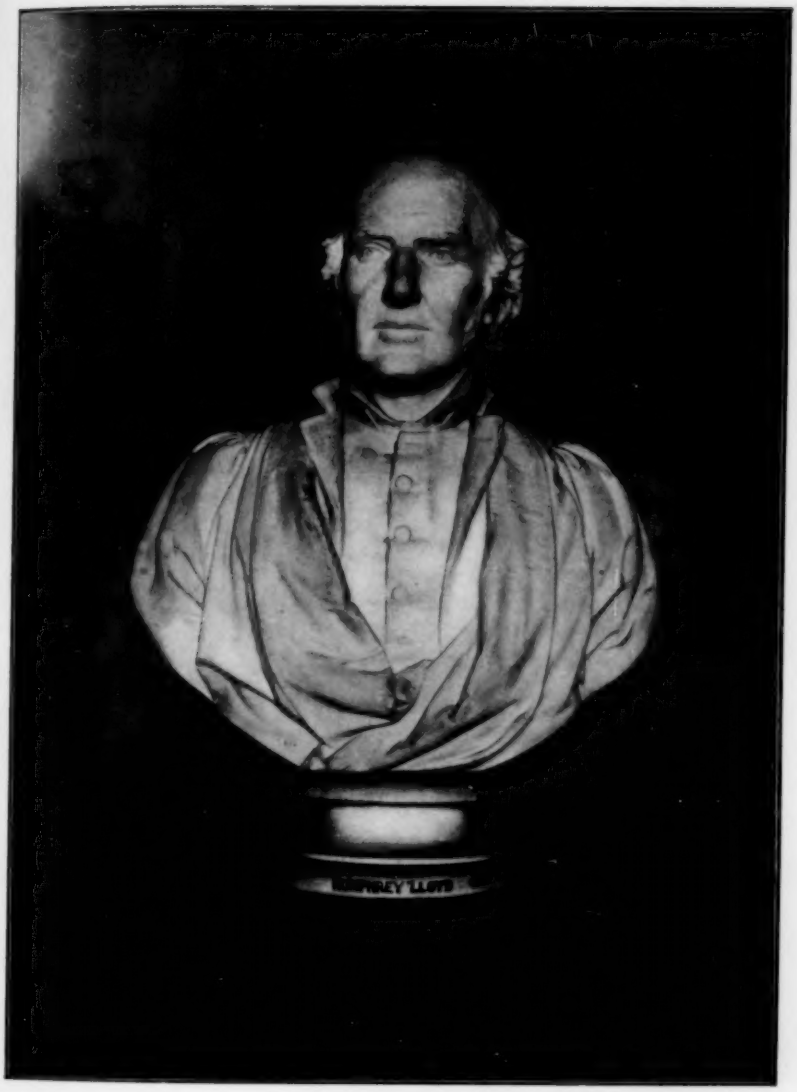
IT is well known that when a ray of light is incident upon certain crystals, such as Iceland spar and quartz, it is in general divided into two pencils, one of which is refracted according to the known law of the sines, while the direction of the other is determined by a new and extraordinary law first assigned by Huyghens.

These laws were long supposed to apply to all doubly-refracting substances; and it was not until the subject was taken up by Fresnel, that the problem of double refraction was solved in all its generality. Setting out from the hypothesis, that the elasticity of the vibrating medium within the crystal is unequal in three rectangular directions, Fresnel has shown that the surface of the wave is neither a sphere nor a spheroid, as in the Huyghenian law, but a surface of the 4th order, consisting of two sheets, whose points of contact with the tangent planes determine the directions of the two rays. From this construction it follows that neither of the rays, in general, obeys the law of Snellius, or that of Huyghens, but that they are both refracted according to a new and more complicated law. Such crystals have two optic axes, and are said to be *biaxal*. When the elasticity of the medium is equal in two of the three directions, the equation of the surface of the wave is resolvable into two quadratic factors, which give the equations of the sphere and spheroid of the Huyghenian theory. The two optic axes in this case coincide in one; and the law of Huyghens is thus deduced from the general solution, and proved to belong to the case of *uniaxal* crystals. Finally, when the elasticity is equal in all the three directions, the surface of the wave becomes a sphere; and the refraction is single, and takes place according to the ordinary law of the sines.

There are two remarkable cases, however, in this elegant and profound theory, which its author seems to have overlooked, if not to have misapprehended. In a communication made, some months since, to the Royal Irish Academy, Professor Hamilton has supplied these omissions in the theory of Fresnel, and has thus been led to results in the highest degree novel and remarkable.

To understand these conclusions, it will be necessary to

* Communicated by the Author.



Bust of HUMPHREY LLOYD (1800-1881)

(Trinity College, Dublin)

examine for a moment the form of the wave. Its equation, referred to polar coordinates, is

$$(a^2\cos^2\alpha + b^2\cos^2 B + c^2\cos^2\gamma)\,r^4$$
$$-[a^2(b^2+c^2)\cos^2\alpha + b^2(a^2+c^2)\cos^2 B + c^2(a^2+b^2)\cos^2\gamma]\,r^2$$
$$+ a^2 b^2 c^2 = 0$$

in which α, B, γ, denote the angles made by the radius vector with the three axes of coordinates. If now we make $\cos\gamma = 0$ in this equation, so as to obtain the section of the surface made by the plane of $x\,y$, the result is reducible to the form,

$$(r^2 - c^2)\,[(a^2\cos^2\alpha + b^2\sin^2\alpha)\,r^2 - a^2 b^2] = 0;$$

so that the surface of the wave intersects the plane of $x\,y$ in a circle and ellipse, whose equations are

$$r = c,\ (a^2\cos^2\alpha + b^2\sin^2\alpha)\,r^4 = a^2 b^2.$$

Now if c, the radius of the circle, be intermediate between a and b, the semiaxes of the ellipse, the two curves will intersect in four points, or *cusps*; and the angle which the radius vector drawn to the cusp makes with the axis of x, is found by eliminating r between the two equations, by which means we obtain

$$\sin\alpha = \pm\frac{a}{c}\sqrt{\frac{c^2-b^2}{a^2-b^2}}.$$

At each of the points thus determined, there will be two tangents to the plane section, and therefore two tangent planes to the surface; and consequently a single ray, proceeding within the crystal to one of these points, will at emergence be divided into two, whose directions are determined by those of the tangent planes.

Such seems to have been Fresnel's conception of this case. Professor Hamilton has shown, however, that there is a cusp at each of these points, not only in this particular section, but in every section of the wave-surface passing through the line whose direction has just been determined; or that there are, in fact, *four conoidal cusps* on the general wave-surface at the points of intersection of the circle and ellipse. So that there must be an infinite number of tangent planes at each of these points, and consequently, a *single ray*, proceeding from a point within the crystal in any of the above-mentioned directions, ought to be divided into an *infinite number of emergent rays*, forming a cone of the 4th order.

It is evident, further, that the circle and ellipse which thus intersect must have four common tangents. Fresnel has shown that the planes passing through these tangents, and parallel to the 3rd or mean axis, are parallel to the circular sections of a curved surface which he calls the surface of elasticity; and he seems to have concluded that these planes touched

the wave-surface only in the two points just mentioned; and consequently that a single ray, proceeding from a point without a biaxal crystal, and refracted in the direction of the optic axis, would necessarily be divided into two, determined by the points of contact. Professor Hamilton, however, has shown that the four planes in question touch the wave-surface, not in two points only, but in an *infinite number of points*, constituting each a small *circle of contact*, whose plane is parallel to one of the two circular sections of the surface of elasticity; and that, consequently, a *single ray* of common light, proceeding from an external point, and refracted in the required direction, ought, if the theory be true, to be divided *within* the crystal into an *infinite number of rays*, constituting a *conical surface*.

Here then are two singular and unexpected consequences of the undulatory theory, not only unsupported by any phænomena hitherto noticed, but even opposed to all the analogies derived from experience. If confirmed by experiment, they would furnish a new and almost convincing proof of the truth of that theory; and if disproved, on the other hand, it is evident that the theory must be abandoned or modified.

Being naturally anxious to submit the theory of waves to this delicate test, and to ascertain how far these new theoretical conclusions were in accordance with actual phænomena, Professor Hamilton requested me to undertake a series of experiments with that view. I accordingly applied myself to this experimental problem with all the attention which the subject so well deserved, and have fortunately succeeded in verifying the first-mentioned species of conical refraction. I hope before long to be able to make similar researches on the second*.

The mineral I employed in these experiments was *arragonite*, which I selected partly on account of the magnitude of the cone which theory indicated in this instance, and partly because the three elasticities in this mineral have been determined, apparently with great care, by Professor Rudberg, and therefore the results of theory could be applied to it at once without further examination. The specimen I used was one of considerable size and purity, procured for me by Mr. Dollond, and cut with its parallel faces perpendicular to the line bisecting the optic axes. If we suppose a ray of common light to pass in both directions out of such a crystal, along the line connecting the two cusps in the wave, it is evident that it must emerge similarly at both surfaces: consequently

* Since we received this paper, we have been informed by the author that he has now *observed* phænomena corresponding to the *second* species of conical refraction, and of which an account will be given in our next Number.—Edit.

the ray which passes along this line, and forms a diverging cone of rays at emergence at the second surface of the crystal, must arise from a converging cone incident upon the first surface. Having therefore nearly ascertained the direction of the optic axis by means of the rings, I placed a lens of short focus at the distance of its own focal length from the first surface, and in such a position that the central rays of the pencil might after refraction pass along the axis. Then looking through the crystal at the light of a lamp placed at a considerable distance, I observed, in the expected direction, a point more luminous than the space immediately about it, and surrounded by something like a stellar radiation. Fearing that this appearance might have arisen from some imperfection in the crystal, I examined it with polarized light, and was happy to find the system of rings in the same direction. This was afterwards confirmed by numerous observations on different parts of the crystal.

This result is of some interest in itself, independently of its connexion with theory. It has been hitherto supposed that the only means of determining experimentally the direction of the optic axes, in substances of weak double refraction, was by observation of the rings which appear around them, when the incident and emergent light is polarized. Here, however, it is seen that common or unpolarized light undergoes such modifications in the neighbourhood of the optic axes of biaxal crystals, that the apparent direction of the axes may be at once determined, and with the simplest contrivance.

But to examine the emergent cone it was necessary to exclude the light which passed through the crystal in other directions. For this purpose a plate of thin metal, having a minute aperture, was placed on the surface of the crystal next the eye; and the position of the aperture so adjusted that the line connecting it with the luminous point on the first surface might be, as nearly as possible, in the direction of the optic axis. The exact adjustment to this direction was made by subsequent trial. The phænomenon which presented itself, on looking through the aperture, when this adjustment was complete, was in the highest degree curious. There appeared a luminous circle with a small dark space round the centre, and in this dark space (which was also nearly circular) were two bright points divided by a narrow and well-defined dark line. When the aperture in the plate was slightly shifted, the appearances rapidly changed. In the first stage of its change the central dark space became greatly enlarged, and a double cone appeared within it. The circle was reduced to about a quadrant, and was separated by a dark interval from

the cone just mentioned. The remote cone then disappeared, and the circular arch diminished; and, as the obliquity of the line to the axis was further increased, these two luminous portions merged gradually into the two pencils into which a single ray is divided in the other parts of the crystal.

The same experiments were repeated by bringing the flame of the lamp close to the first surface of the crystal. In this case the lens was removed, and the incident cone of rays formed by covering the surface of incidence with a thin metallic plate perforated with a minute aperture. The results were perfectly similar to those obtained in the former case.

But to apply a yet more palpable test to this theory, I substituted a narrow linear aperture for the point, in the plate next the lamp; and fixed it so that the plane passing through the line on the first plate and the point in the second, should be the plane of the optic axes. In this case, according to the received theory, all the rays transmitted through the two apertures should be refracted doubly in the plane of the optic axes, so that no part of the line should appear enlarged in breadth on looking through the second aperture; whereas, according to Professor Hamilton's beautiful deduction from the same theory, the ray proceeding in the direction of an optic axis should be refracted in every plane passing through that line. In accordance with this conclusion I found, on looking through the second aperture, that the luminous line was undilated, except in the direction of one of the optic axes; and that in the neighbourhood of this direction its boundaries ceased to be rectilinear, and it swelled out into an oval curve.

This experiment seems to go far in affording a general verification of the principle. I was anxious, however, to observe the emergent cone more directly. After some trials I effected this with the sun's light, and received the rays, emerging from the aperture in the second plate, on a screen of roughened glass. I was thus enabled to observe the phænomenon at various distances, and with all the advantages of enlargement. The light was sufficiently bright, and the appearance distinct, when the plane section of the cone of rays on the screen was even two inches in diameter.

Examining the emergent cone with a tourmaline plate, I was surprised to observe that one radius only of the section of the cone vanished, in a given position of the axis of the tourmaline; and that the ray which disappeared ranged through 360°, as the tourmaline plate was turned through 180°. From this it appeared that all the rays of the cone are polarized in different planes.

On examining this curious phænomenon more attentively,

I discovered the remarkable law,—that "the angle between the planes of polarization of any two rays of the cone is half the angle contained by the planes passing through the rays themselves and its axis." This law accounts for the disappearance of one radius only of the section of the cone, the opposite radius being in fact polarized in a plane at right angles to the plane of polarization of the first. The law itself can be easily shown to be a necessary consequence of the general theory applied to this particular case; it is, however, but approximately true, and holds only on the assumption that the biaxal energy of the crystal is small,—an assumption justified by the phænomena of all crystals hitherto examined.

The general phænomena being observed, it remained to take measurements, and to compare them with the results of theory. For this purpose I determined the magnitude of a section of the cone, at a considerable distance from the crystal, by observing, with the assistance of a small telescope, the points at which the aperture ceased to be visible by means of the transmitted light. The distance being then accurately measured, the angle of the cone could be obtained from a table of tangents. This angle was thus found to amount to $6° 14'$ in the plane of the optic axes, and to $5° 46'$ in the perpendicular plane,—the mean being exactly $6°$. I then placed the flame of a wax taper at the centre of this section, and removing the plate from the second surface of the crystal, placed a mark at a considerable distance on the line of the reflected ray. Then placing a Hadley's sextant with its centre in the place of the crystal, I measured the angular distance between the flame and the mark. This angle was found to be $31° 56'$, and consequently the angle of emergence corresponding to the axis of the cone was $15° 58'$.

Now assuming the three indices for arragonite to be 1·5926, 1·6863, 1·6908, which are the indices for the mean ray E, as determined by Professor Rudberg*, Professor Hamilton has shown that the direction of the emergent rays in the plane of the optic axes will be given by the formulæ

$$\sin R_o = 1{\cdot}6863 \,.\, \sin I$$
$$\sin R_e = 1{\cdot}68708 \,.\, \sin (I - 1° 44' 48'')$$

in which I is the internal angle of incidence, or the angle which the cusp ray makes with the normal to the surface of emergence; and R_o, R_e are the corresponding angles of refraction in air. But in the present instance the normal to the surface of emergence bisects the angle of the optic axes, and therefore $I = 9° 56' 27''$. Consequently $R_e = 16° 55' 27''$,

* See Lond. and Edinb. Phil. Mag. and Journ., vol. i. p. 140—141.—Edit.

and $R_2 = 13° 54' 49''$. Now the difference of these angles, or $3° 0' 38''$, may be called the angle of the cone; and half the sum, or $15° 25' 8''$, is the mean angle of emergence. The angle $15° 58'$, found above, differs from this by $33'$ only; but the observed angle of the cone is about double of that given by theory.

I also measured the angle of the cone by receiving it on a screen of roughened glass at different distances, and tracing the outline of the section on the screen: the diameter of this section and the distance being then measured, the angle was determined. Three measurements taken in this manner gave for the magnitude of that angle respectively $6° 24'$, $5° 56'$, $6° 22'$, the mean of which, $6° 14'$, agrees very nearly with that determined by the former method.

Conceiving that the difference between experiment and theory arose chiefly from the rays which were inclined to the optic axis all round at small angles, and which were transmitted at the second surface in consequence of the sensible magnitude of the aperture, I determined to try the effects of apertures of various forms and dimensions.

When the aperture was at all considerable, two concentric circles were seen to surround the optic axis, the interior circle having about double the brightness of the annulus which surrounded it. The light of the interior circle was unpolarized, while that of the surrounding annulus was polarized according to the law already mentioned. When the aperture was diminished, the inner circle contracted in diameter, the breadth of the outer annulus remaining nearly the same; until the former was finally reduced to a point in the centre of the exterior circle. When the aperture was still further diminished, a dark space sprung up in the centre, which enlarged as the aperture decreased; until finally, with a very minute aperture, I succeeded in rendering this space about $\frac{3}{4}$ of the whole, or of reducing the breadth of the luminous annulus to about $\frac{1}{8}$ of its exterior diameter.

With this diminished aperture I examined the appearance produced by a line of light on the first surface parallel to the plane of the optic axes. The swelling curves, which it has been already remarked, surrounded the optic axis in this case, were reduced to a breadth corresponding to that of the annulus in the former experiment, and were separated by a considerable dark interval. When the plane passing through the two apertures deviated a little from the plane of the optic axes, the phænomena underwent many beautiful changes, the curves assuming in all cases the form of the conchoid, whose pole was the projection of the optic axis, and asymptote the line on the first surface.

Finally, when the apertures on the two surfaces were transposed, I found that no change was made in the resulting phænomena; and that they seemed, in fact, to be in all respects similarly related to the surfaces of incidence and emergence.

It is easy to render an account of these various appearances. When the aperture on the second surface is considerable, the rays proceeding to its circumference from a point on the first surface will be sensibly inclined to the optic axis, which we shall suppose to be in the line connecting the point with the centre of the aperture. Consequently the interior, as well as the exterior rays, into which each of them is divided, will be inclined *outwards*; and it is obvious that there will be a central bright space, every point of which is illuminated by one interior and one exterior ray. This space therefore will have double the brightness of the surrounding space, each point of which is illuminated by one ray only; and as the rays which combine to form it are polarized in planes at right angles to one another, the resulting light will be unpolarized.

When the aperture is diminished, the inclination of these interior rays to one another decreases; until finally they become parallel, and the central bright space is reduced to a point. When the aperture is still further diminished, the interior rays become inclined *inwards*, and cross; and it is obvious that beyond the point of junction there will be a dark space illumined by no ray whatever. As there is no meeting of rays oppositely polarized in this case, the whole of the light will be polarized, and according to the law already explained. Finally, when the aperture is still further diminished, the interior rays at one side approach to parallelism with the exterior at the other; and the central dark space enlarges, and approaches to equality with the outer and limiting cone. Thus the annulus of light is indefinitely diminished in breadth, and the cone approaches to a mathematical surface.

It will be easily seen that the angle of the true cone is, nearly, half the sum of the angles of the exterior and interior limits of the observed conical annulus; and that when a bright space appears in the centre, as is the case when the aperture is considerable, the true angle is half the difference of the angles of the interior and exterior cones. When the whole cone is of uniform brightness, and the central dark space reduced to a point, the observed cone is just double of that sought.

Now this last was very nearly the case in the experiments from which the measures already mentioned were taken; and consequently the corrected angle, being in this case half the observed, coincides very nearly with that deduced from theory.

As there must be an equal cone of rays incident upon the first surface of the crystal, I took other measurements with a view to determine its magnitude. For this purpose I placed a rough micrometer, consisting of two moveable metallic plates, immediately before the lens, and closed the plates, until on looking through the aperture on the second surface I could see them just touching the opposite sides of the circular images. The same thing was done for the interior circle of the annulus, and the focal length of the lens accurately measured. In this manner the extreme dimensions of the conical annulus were ascertained, and the true angle calculated. The mean of three such measurements gave 3° 47′ for the corrected angle of the cone.

It has been observed that the theoretical angle of the cone has been computed from the three indices of refraction as determined by M. Rudberg. Now a very small error in the determination of these indices, or a very minute difference between their values in different specimens of the same mineral, would make a considerable change in the angle. On the other hand, the effects of diffraction must in some degree modify the experimental results. And hence, though the measures were not taken with all the means to insure accuracy of which they are susceptible, it will be seen that their correspondence with theory is as close as could be reasonably expected.

Gilbert, Bacon, Galilée, Képler, Harvey et Descartes : leurs relations.

VOLTAIRE s'est indigné (1) que DESCARTES n'ait jamais cité GALILÉE. Cette remarque, en dépit du ton assuré de l'auteur et de la véhémence de ses blâmes, est si bien faite pour nous stupéfier que le lecteur ne peut s'empêcher d'en vouloir vérifier l'exactitude. Mais le contrôle des textes ne permet aucun doute, VOLTAIRE avait raison : DESCARTES n'a mentionné dans son œuvre ni GALILÉE, ni KÉPLER, à qui pourtant il devait tant, et il convenait dès lors de rechercher les motifs de son silence.

A mesure que nous tentions, aussi bien de comprendre l'essence des divergences qui éloignèrent DESCARTES de GALILÉE que de pénétrer la nature intime de leurs affinités, nous vîmes tout l'intérêt qu'il y aurait à étendre également cette étude, comme nous le suggérait Mr. SARTON, à quatre de leurs contemporains les plus illustres. On trouvera ici une étude systématique des relations qu'eurent — éventuellement — entre eux GILBERT, BACON, GALILÉE, KÉPLER, HARVEY et DESCARTES.

Un tel travail au premier abord pouvait être envisagé comme n'étant seulement qu'un chapitre de l'histoire des idées. Toutefois, je me rendis bientôt compte que ce point de vue devait être modifié. En effet, il est rare que l'attitude d'un savant à l'égard d'un autre ne dépende uniquement que de la dette intellectuelle qu'il a contractée ; dans une bien plus grande mesure, elle reflète surtout sa personnalité, oserai-je dire sa physionomie ? et cela, d'autant plus que, comme on le verra, cette attitude ne varie généralement pas dans le temps. Le fait que l'auteur du *Discours de la Méthode* n'a cru devoir citer GALILÉE et KÉPLER que dans ses lettres n'est-il pas susceptible de nous éclairer sur les tendances profondes qui

(1) *Siècle de Louis XIV*, chap. 31, des Sciences.

animaient le philosophe et de nous apporter sur lui des renseignements aussi véritables et peut-être plus riches de signification que ceux obtenus par un dépistage parfois hasardeux des héritages intellectuels ? Le silence de DESCARTES vaut d'être étudié pour lui-même et a une valeur en soi qui mérite d'être signalée. Nous avons donc été amenés à considérer avant tout les relations directes des savants énumérés ci-dessus et les jugements mutuels qu'ils émirent les uns sur les autres. Nous indiquerons avec plus de précision dans nos conclusions finales les enseignements qu'on peut tirer d'une telle façon d'examiner les choses.

Je crois devoir mentionner au seuil de cette étude deux obstacles qui se sont présentés et qui risquaient de fausser la perspective de nos appréciations : tout d'abord, l'on est bien obligé d'admettre que, la gratitude mise à part, il n'y a souvent aucun motif raisonnable qui puisse amener un savant à reconnaître sa dette et à s'exprimer sur les influences qu'il a subies. D'autre part, au dix-septième siècle, mais moins qu'au siècle précédent, il arrive qu'un auteur ne cite pas le nom de celui à qui il emprunte une idée ; il fait grand étalage d'érudition et a soin de n'énumérer en réalité que les ouvrages auxquels il ne doit rien.

Dans la plupart des cas, il a été possible de lever ces deux difficultés. Au point de vue de l'ordre à adopter dans le présent travail, il pouvait sembler naturel de se conformer à la chronologie, principalement à celle des publications les plus importantes de nos auteurs. C'est ainsi que nous aurions étudié successivement : GILBERT (1540-1603), BACON (1561-1626), GALILÉE (1564-1642), KÉPLER (1571-1630), HARVEY (1578-1657) et DESCARTES (1596-1650). Toutefois, il nous a paru commode de grouper les deux philosophes ensemble et de placer KÉPLER avant GALILÉE. En ce qui concerne GILBERT, il ne fallait évidemment pas s'attendre à trouver dans son *De Magnete* (1600) des références à nos autres auteurs (2), et nous n'aurons plus loin qu'à examiner son influence et les critiques dont son œuvre a été l'objet. (3)

(2) « There is no trace of any acquaintance between GILBERT and BACON, therefore we must admit that there is nothing to show that the last can have affected the elder » : W. HALE-WHITE, *Bacon, Gilbert and Harvey ;* London, 1927, p. 45.

(3) Outre le traité *De Magnete*, nous avons examiné encore : *The Making, Description, and use of two most ingenious and necessarie Instruments for Sea-men...* in *The Theoriques of the Seuen Planets...* by (THOMAS) BLUNDEUILE... 1602 et *De Mundo nostro Sublunari Philosophia Nova.* Opus posthumum... (1651).

BACON (*)

Je me propose de consacrer prochainement une étude à la réputation dont jouit BACON auprès des savants, depuis le milieu du dix-septième siècle jusqu'à l'époque de VOLTAIRE. On verra qu'une enquête menée parmi les principaux représentants de la pensée scientifique durant cette période, dont — après les tâtonnements du début du grand siècle — le rôle fut décisif dans la formation et la consolidation définitives de la science moderne, apporte une justification nouvelle de l'opportunité et du bien-fondé de l'attaque violente dirigée autrefois par LIEBIG contre BACON. (4)

Il n'y a plus guère aujourd'hui que les historiens de la philosophie pour croire encore à l'influence du « père de la philosophie expérimentale », comme écrivait naïvement VOLTAIRE (5), sur la marche des sciences. « La vérité, dit LIEBIG (6), c'est que BACON ne se doutait nullement ni des pensées actives qui, à son époque, étaient en mouvement dans le domaine des sciences, ni des gigantesques travaux accomplis par ses propres contemporains ». Et il ajoute : « De tous ces grands travaux et résultats auxquels se rattachent ceux de notre époque actuelle, comme à autant de premiers anneaux d'une longue chaîne non interrompue, BACON ne comprit et ne sut absolument rien » (7). C'est ce que confirment pleinement aussi les conclusions de notre travail, comme on va le voir.

BACON ET DESCARTES

BACON mourut en 1626, c'est-à-dire bien avant que DESCARTES eût encore rien publié. En ce qui concerne l'influence de BACON sur DESCARTES, voir ci-dessous le paragraphe : DESCARTES et BACON.

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* Pour l'étude de BACON, j'ai fait usage de l'édition suivante : *The Works of Francis Bacon* (SPEDDING, ELLIS and HEATH), Boston, vol. I (1861) à vol. 10 (1864). Toutes les citations dans ce chapitre se rapportent à cette édition, les chiffres romains indiquant le volume, les chiffres arabes la page.

(4) J. VON LIEBIG, *Lord Bacon;* trad. de l'allemand par P. DE TCHIHATCHEF, Paris, GUÉRIN, 1866.

(5) VOLTAIRE, *Œuvres complètes*, t. 22, 1879; 12e lettre philosophique, p. 118.

(6) LIEBIG. *loc. cit.*, pag. 82.

(7) *Id.*, p. 85.

BACON ET GILBERT

Il n'est pas aisé d'ajouter quelque chose à ce qu'a dit LIEBIG (8), si ce n'est de nouvelles références qui montrent combien sa sévérité était fondée.

BACON tantôt plagie GILBERT sans le nommer, comme dans la *Inquisitio de Magnete* ou l'*Historia ventorum*, tantôt les découvertes du physicien anglais sont taxées de fables (I, 494, 260 et 268) et ses opinions délibérément repoussées (V, 337; VII, 348, 300 et 307) ou prises en considération, mais sans sympathie (I, 435). BACON lui dénie le mérite de la nouveauté (VII, 330), le traite par dessous la jambe (II, 140) et se gausse de lui : « ... vires magneticas non inscite introduxit Gilbertus, sed et ipse factus magnes; nimio scilicet plura quam oportet ad illas trahens, et navem aedificans ex scalmo » (III, 300). Certes, BACON reconnaît un certain mérite à GILBERT, pour avoir renouvelé par exemple les dogmes de PHILOLAÜS (II, 287), mais les éloges qu'il lui accorde parfois (et indirectement) pour des suggestions (II, 232; VII, 334 et 306) semblent souvent décernés à regret, car il n'est pas rare qu'il s'empresse de les faire suivre de réserves et de critiques d'ailleurs parfaitement injustifiées (V, 263) : « Verum haec, licet bene observata, tamen non evincunt prorsus quod ille (Gilbertus) asserit » (I, 445). BACON n'hésite pas à ridiculiser son compatriote : « Statim imminebat et ipse novae philosophiae condendae; nec Xenophanis nomen in Xenomanem per ludibrium versum expavit, in cujus sententiam inclinabat » (VII, 75). Le passage suivant, très analogue, achèvera d'édifier le lecteur : « Quin et nuper Gilbertum nostratem, cum naturam Magnetis laboriosissime et magna judicii firmitudine et constantia, necnon experimentorum magno comitatu et fere agmine perscrutatus esset, statim novae in Philosophia Naturali sectae imminere coepisse; nec Xenophanis nomen in ludibrium versum expavisse, in cujus sententiam inclinabat. Hos itaque, et si qui sunt aut erunt horum similes, antiquorum turbae aggregandos; unam enim eandemque omnium rationem haberi. Esse nimirum homines secundum pauca pronuntiantes, et naturam leviter attingentes, nec ita se illi immiscentes

(8) *Loc. cit.* : passim.

ut aut contemplationum veritatem aut operum utilitatem assequi possint » (VII, 118-9).

On reconnaîtra donc qu'on peut appliquer à BACON son propre jugement sur ARISTOTE : « ... per omnia sibi author, quique antiquitatem ita despexit, ut neminem ex antiquis vel nominare fere dignetur, nisi ad confutationem et opprobrium » (VII, 70).

BACON ET GALILÉE

BACON et DESCARTES, l'un et l'autre, ont eu envers GALILÉE la même dédaigneuse attitude. « Le nom de GALILÉE, écrivait LIEBIG (9), ne se rencontre dans les ouvrages de BACON que deux fois, et, dans les deux cas, cette mention n'est faite qu'à l'occasion de ce que lui écrivait d'Italie, relativement à GALILÉE, M. MATHEW, qui avait traduit en italien les *Essais* de BACON ». Et plus tard, LIEBIG ajoutait : « Tout ce que BACON connaît de GALILÉE est résumé dans les deux passages indiqués par moi » (10). Il s'agit de *Sylv. Sylv.* 791 et *Nov. Org.* II. Aph. 39.

GALILÉE avait fait paraître son *Sidereus Nuncius* en 1610, mais notre réformateur en a si peu entendu parler qu'on trouve ceci sous sa plume, dans la *Descriptio Globi intellectualis*, qui date probablement de 1612 environ :

« Facile autem quis cernat, et eos quibus terram rotari placet, et eos contra qui primum mobile et veterem constructionem tenuerunt, aequa fere et ancipiti phaenomenorum advocatione niti. Quin et ille novae constructionis nostra aetate author, qui solem secundi mobilis, quemadmodum terram primi mobilis, centrum constituit, ut planetae in propriis suis conversionibus circa solem choreas ducere videantur (quod ex antiquioribus nonnulli de Venere et Mercurio suspicati sunt), si cogitata ad exitum perduxisset, belle profecto rem conficere potuisse videtur. Neque vero nobis dubium est, quin et aliae hujusmodi constructiones ingenio et acri cogitatione adinveniri possint. Neque illis qui ista proponunt admodum placet, haec quae adducunt prorsus vera esse, sed tantummodo ad computationes et tabulas conficiendas commode supposita. At nostra ratio alio spectat; non enim concinnationes, quae variae esse possunt, sed veritatem rei quaerimus, quae simplex est. Ad hoc vero historia phaenomenorum sincera viam aperiret, infecta dogmate obstruet » (VII, 295-6).

C'est à dessein que j'ai allongé cet extrait, abondant jusqu'à l'indiscrétion, car les dernières lignes caractérisent très bien le

(9) *Loc. cit.*, p. 81, note.
(10) *Id.*, p. 160.

procédé habituel de BACON. Voici ce qu'on lit dans le *Novum Organum* (environ 1620) :

« Hinc maculae in sole, et id genus : omnia certe inventa nobilia, quatenus fides hujusmodi demonstrationibus tuto adhiberi possit. Quae nobis ob hoc maxime suspectae sunt, quod in istis paucis sistatur experimentum, neque alia complura investigatu aeque digna eadem ratione inventa sint » (I, 457).

BACON donne une fausse interprétation de la théorie des marées de GALILÉE (1616), qu'il n'a visiblement pas comprise. Au surplus, le manque d'informations suffisantes qu'il reproche au savant italien est révélateur de la manière superficielle et du manque de pénétration du Chancelier :

« Etiam ex hoc fundamento inaequalitatis motuum quoad velocitatem, excogitavit Galilaeus causem fluxus et refluxus maris; rotante terra velocius, aquis tardius; ideoque accumulantibus se aquis in sursum, et deinde per vices se remittentibus in deorsum, ut demonstratur in vase aquae incitatius movente. Sed hoc commentus est concesso non concessibili (quod terra nempe moveatur), ac etiam non bene informatus de oceani motu sexhorario » (I, 484-5).

En 1622, BACON n'hésite pas à écrire ceci :

« Neque ex recentioribus quisquam operae pretium circa hoc fecit, addendo solummodo pauca mechanica, eaque per demonstrationes suas detorta » (III, 300).

Les éloges sont négligeables. N'est-ce pas une impertinence à l'égard de GALILÉE qui vient de donner au monde son *Sidereus Nuncius* que de le louer d'avoir contribué — avec d'autres — au progrès de la sélénographie (VII, 332)? D'ailleurs, BACON ne manque pas de mettre en doute la réalité des découvertes du savant italien (VII, 333, 340 et 346). Les acquisitions nouvelles le déroutent : il se dérobe et évite de se livrer à une critique réellement scientifique en prétextant le manque de diligence des auteurs à procéder à de patientes observations, ou encore en invoquant les conditions que lui imposent ses bien plus grands travaux : « *nos vero nulla cum priorum inventorum jactura, tamen longe majus opus movemus* » (VII, 295).

BACON ET KÉPLER

BACON a ignoré complètement les découvertes de KÉPLER; ajoutons qu'en général on ne rencontre presque jamais chez lui

la mention d'auteurs allemands (11). Même dans la *Descriptio Globis intellectualis* (environ 1612) qui est cependant de tous ses ouvrages celui où il a fait la part la plus considérable à l'astronomie, on ne trouve aucune mention des deux premières lois des mouvements planétaires, publiées en 1609 dans le *De Stella Martis*; ce dernier ouvrage était pourtant connu en Angleterre dès 1610; plus tard, BACON ignorera tout autant la troisième loi de KÉPLER (découverte en 1618 et publiée en 1619 dans les *Harmonices mundi libri V*). Mais comment aurait-il pu accueillir les idées de KÉPLER alors que son opposition au système de COPERNIC devenait avec le temps de plus en plus marquée? N'écrivait-il pas en 1623 :

« Veniamus nunc ad eam scientiam ad quam nos ducit oraculum antiquum ; nempe ad scientiam nostri. Cui, quo magis nostra intersit, eo incumbendum est diligentius. Haec scientia Homini pro fine est scientiarum ; at Naturae ipsius portio tantum. Atque hoc pro regula ponatur generali; quod omnes scientiarum partitiones ita intelligantur et adhibeantur, ut scientias potius signent aut distinguant quam secent et divellant; ut perpetuo evitetur Solutio Continuitatis in Scientiis. Hujus etenim contrarium particulares scientias steriles reddidit, inanes, et erroneas; dum a fonte et fomite communi non aluntur, sustentantur, et rectificantur. Sic videmus Ciceronem oratorem de Socrate et ejus schola conquerentem, quod hic primus Philosophiam a Rhetorica disjunxerit; unde facta sit Rhetorica ars loquax et inanis... Constat similiter sententiam Copernici de Rotatione Terrae (quae nunc quoque invaluit), quia phaenomenis non repugnat, ab Astronomicis Principiis non posse revinci; a Naturalis tamen Philosophiae Principiis, recte positis, posse » (II, 310-1).

Ce passage, dans le ton habituel de BACON, n'autorise qu'une conclusion : son auteur fut un « dilettante ignorant et effronté » (12).

BACON ET HARVEY

HARVEY n'a jamais été cité par BACON. D'autre part, selon AUBREY (13), le Chancelier aurait été soigné par HARVEY. Celui-ci avait commencé dès 1616 à enseigner la circulation du sang, mais, pour on ne sait quelles raisons, ses leçons n'eurent apparemment aucun retentissement et il est possible que ce soit en cette année aussi qu'il devint le médecin de BACON (14). Quoiqu'il

(11) Les noms de VÉSALE et de STÉVIN lui paraissent inconnus aussi.
(12) LIEBIG, *loc. cit.*, p. 249.
(13) *Brief Lives*, edit. A. CLARK, Oxford, 1898; I, 299 et 72.
(14) W. HALE-WHITE, *Bacon, Gilbert and Harvey;* London 1927; p. 19.

en soit, pendant les dix dernières années de sa vie (1616-1626) BACON écrit comme s'il ne paraissait pas avoir entendu parler de la découverte de son compatriote. « Le silence, commentait LIEBIG, qu'il garde à l'égard de HARVEY, dont le nom est banni de ses ouvrages, est éminemment significatif » (15). Bien plus, suivant LIEBIG toujours, l'*Historia vitae et mortis*, publiée en 1623, « est très vraisemblablement dirigée contre la personne de HARVEY » (16). Il n'est pas douteux que LIEBIG se montre exagérément sévère ici, car, comme a bien voulu me l'apprendre Mr. CH. SINGER dans une lettre récente, il n'existe en effet aucun témoignage direct qui nous porte à croire que l'on eût connaissance avant 1628 des idées de HARVEY sur la circulation; on possède en outre des preuves indirectes qu'elles étaient ignorées même de ses collaborateurs les plus proches. Il y a donc mauvaise grâce à reprocher au Chancelier de s'être tu dans le cas présent et il n'est pas sans intérêt de rappeler d'autre part, à propos du manque de publicité fait aux découvertes sensationnelles exposées par HARVEY dans ses leçons, qu'une aventure semblable advint à NEWTON lorsque ses travaux sur la réfrangibilité de la lumière ne furent connus que par son premier mémoire du 19 février 1671/2 (Ph. Trans. N° 80), bien qu'il eût exposé le résultat de ses expériences dès janvier 1669/70 dans son cours à l'université de Cambridge.

DESCARTES (*)

DESCARTES ET BACON

En ce qui concerne les relations des deux philosophes, nous n'avons rien à ajouter ici après la pénétrante étude que G. MILHAUD a consacrée à ce sujet (17) et où il a reproduit (18) les passages qui chez DESCARTES ont trait à BACON (le nom du Chancelier n'est cité que trois fois, dans la correspondance).

MILHAUD a montré que DESCARTES non seulement ne paraît

(15) LIEBIG, *loc. cit.*, p. 101.

(16) *Id.* p. 99.

(*) Pour l'étude de DESCARTES, j'ai fait usage de l'édition ADAM-TANNERY: *Œuvres de Descartes*, 12 vol. plus un supplément, Paris, 1897-1913.

(17) G. MILHAUD, *Descartes Savant*, Paris, ALCAN, 1921, chap. 10, p. 213-227.

(18) D'après l'édition ADAM-TANNERY.

pas avoir eu conscience de cette « opposition en apparence irréductible » entre BACON et lui qui a été formulée depuis par l'histoire, mais que bien au contraire il aurait nourri une « indulgente sympathie » à l'égard du philosophe anglais, dont les conseils « traduisaient le plus souvent à leur manière les règles de la méthode cartésienne » (19).

Il convient de mentionner qu'une lettre de CONSTANTIN HUYGENS (20) (25 janvier 1642) que n'a pu connaître MILHAUD nous apporte une confirmation des remarques de l'historien français sous la forme d'une allusion à une conversation du père du grand HUYGENS avec DESCARTES : « Pour ce qui est du Traicté de l'Aymant que voyci (ouvrage non identifié), il (MERSENNE) m'auoit authorisé de le lire auant que m'en defaire; ... en general l'intention de l'auteur est fort perceptible; et ne me repens point de l'auoir parcourru. Il est vray que ce qu'il appelle demonstration ne me semble pas tousiours l'estre, mais j'ay creu en lisant, pour estre sorti depuis peu de la lecture de voz Meditations, que c'est là où j'ay gaigné le goust si delicat, qu'il n'y a plus de viande ordinaire qui me puisse satisfaire. Outre que les experiences qu'il produict, et dont quelques unes m'ont esté incognues jusques ores, m'ont semblé meriter le peu de temps que j'y ay mis. Et je me souuiens que ce fut une des causes pour lesquelles vous me distes un jour regretter la mort du Baron Verulam, de le veoir si soigneux et liberal en experiments particuliers ».

DESCARTES ET GILBERT

M. ADAM nous assure que « DESCARTES se tenait au courant : il avait lu autrefois le livre déjà classique du savant anglais GILBERT » (XII, 393). D'autre part, Mr. LORIA se demande « comment justifier le fait qu'il (DESCARTES) n'a jamais montré qu'il connût le célèbre opuscule de W. GILBERT, *De Magnete* » (21). Ces deux opinions ne sont pas contradictoires. Regrettons seulement que Mr. ADAM ne nous ait pas donné les raisons de son assertion. Il n'y a dans

(19) MILHAUD, *loc. cit.*, p. 222.

(20) LÉON ROTH, *Correspondence of Descartes and Constantyn Huygens*, 1635-1647; Oxford, 1926; pp. 162-3.

(21) *Scientia*, t. 39, p. 111 supplém.

toute l'œuvre de DESCARTES (y compris la correspondance) que deux références à GILBERT. Si l'on assigne l'année 1628 à la composition des *Regulae*, la première en date des deux citations est à peine intéressante car elle ne nous éclaire pas sur la date exacte de la lecture par DESCARTES du *De Magnete*. Quant aux interprétations dont ce passage est susceptible, elles peuvent revêtir, me semble-t-il, des significations si opposées que je préfère laisser au lecteur le soin de porter une appréciation sur le jugement de DESCARTES :

« ... ut quaestio sit perfecta, volumus illam omnino determinari, adeo ut nihil amplius quaeratur, quam id quod deduci potest ex datis : ut si petat aliquis a me quid de natura magnetis sit inferendum praecise ex illis experimentis, quae Gilbertus se fecisse asserit, sive vera sint, sive falsa » (X, 431) (22).

La seconde mention du physicien anglais se trouve dans les *Principes* (1644); bien que cinquante et un articles de la quatrième et dernière partie soient consacrés à une étude des propriétés de l'aimant, DESCARTES n'est amené qu'incidemment à citer GILBERT, « qui a découuert le premier que toute la Terre *est un aymant*, & qui en a tres curieusement examiné les vertus » (IX, 295).

L'attitude de DESCARTES vis-à-vis de l'auteur du traité *De Magnete* peut être expliquée par quelques-unes des raisons que nous donnerons pour justifier ce qu'il a écrit de GALILÉE et de KÉPLER, ou, plus exactement, ce qu'il n'a pas dit d'eux. Ajoutons qu'il semble que le mérite accordé aujourd'hui à GILBERT ne lui avait pas encore été unanimement reconnu. C'est ainsi que, selon BAILLET (23), LAURENT REAEL « passoit pour le prémier homme du siécle dans la Philosophie *magnétique*; & GILBERT ny CABEUS n'avoient rien à luy apprendre sur ce sujet ».

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(22) On lit dans une lettre à MERSENNE du 29 janvier 1640 : « I'ay sceu, il y a longtemps, toutes les Experiences de l'Ayman dont vous m'écriuez, & puis aisement donner raison de toutes dans mon Monde » (III, 8).

(23) ADRIEN BAILLET, *La vie de Monsieur Descartes*, Paris, 1691 : 1re partie, p. 319. REAEL, né et mort à Amsterdam (1583-1637), vice-amiral et gouverneur général des Indes Orientales; les charges de la vie publique ne l'empêchèrent pas de cultiver la physique avec succès. Il fit partie d'une commission chargée d'examiner l'offre faite par GALILÉE aux États généraux de sa découverte de la détermination des longitudes (II, 100). Il existe trois lettres de GALILÉE à REAEL et deux de REAEL à GALILÉE.

DESCARTES ET KÉPLER

Très probablement, DESCARTES n'a jamais rencontré KÉPLER. Dans *Les Fondateurs de l'Astronomie Moderne* (24) JOSEPH BERTRAND nous assure (p. 132) que « DESCARTES, qui le (le traité *Paralipomena in Vitellionem*, de KÉPLER, 1604) cite avec honneur dans sa *Dioptrique*, reconnaît expressément le parti qu'il en a tiré ». Si l'éminent secrétaire perpétuel de l'Académie des sciences, mathématicien hors ligne mais historien d'occasion, avait pris soin de contrôler ses affirmations, il aurait constaté avec surprise que non seulement le nom de l'auteur des *Paralipomena* et de la *Dioptrice* (1611) ne figure pas dans *La Dioptrique* (1637), mais qu'il n'a jamais été cité dans ses œuvres par DESCARTES. Pour trouver la mention de KÉPLER, c'est à la correspondance du philosophe qu'on doit s'adresser. Mais ici aussi, quelle déception. Car bien qu'il soit évident par la correspondance que le jeune savant français possédait dès 1626 l'essentiel de sa *Dioptrique*, les références à KÉPLER se bornent à nous révéler qu'en mars 1630 (I, 127) (25) DESCARTES avait déjà lu le *De Nive Sexangula*, court traité publié en 1611 (26) et en 1635 au plus tard (I, 331) très vraisemblablement la *Dioptrice* (1611) (27). Ce sont là renseignements peu significatifs en apparence; indications qui cependant commencent à prendre leur pleine valeur lorsque, comme on le verra plus loin, une lecture superficielle des textes nous laisse tout d'abord indécis sur l'impression que firent sur Descartes les lois du mouvement des planètes.

A la suite de la publication de sa propre *Dioptrique*, DESCARTES semble avoir été accusé de plagiat. Voici comment il s'en défend auprès du Père MERSENNE (31 mars 1638) : « Celuy qui m'accuse (28) d'auoir emprunté de KEPLER les Ellipses & les Hyperboles de ma Dioptrique, doit estre ignorant ou malicieux;

(24) PARIS, Hetzel, 3me édit., 1865 (?).

(25) MILHAUD a montré (*Descartes savant*, 1921, p. 99) qu'en 1618-19, DESCARTES ne connaissait rien encore des travaux de KÉPLER.

(26) FRISCH, VII, 715-730.

(27) MILHAUD a émis l'hypothèse que l'on pourrait peut-être dater du 11 novembre 1620 la première lecture de la *Dioptrice* (*Descartes savant*, p. 102).

(28) Il n'est pas possible de connaître l'auteur de cette protestation, la lettre du P. MERSENNE qui rapporte cette accusation faisant défaut.

car pour l'Ellipse, ie n'ay pas de memoire que KEPLER en parle, ou s'il en parle, c'est assurement pour dire qu'elle n'est pas l'anaclastique (29) qu'il cherche; et pour l'Hyperbole, ie me souuiens fort bien qu'il pretend demonstrer expressement qu'elle ne l'est pas, bien qu'il die qu'elle n'en est pas beaucoup differente. Or ie vous laisse a penser si ie doy auoir emprunté une chose d'un homme qui a tasché de prouuer qu'elle estoit fausse. Cela n'empesche pas que ie n'auouë que KEPLER a esté mon 1^er^ maistre en Optique, & que ie croy qu'il a esté celuy de tous qui en a le plus sceu par cy deuant » (II, 85-86). A part une autre référence à KÉPLER (IV, 398), c'est tout. DESCARTES ne parle pas davantage de l'astronome allemand (30). Il convient donc de se demander ce qu'une attitude aussi singulière signifie en réalité. Un exemple saisissant va nous le révéler.

Sans vouloir rechercher si DESCARTES a pu emprunter à KÉPLER l'idée des tourbillons et l'explication de la pesanteur, proposons-nous d'examiner l'accueil qu'il fit aux lois des mouvements planétaires.

Qu'il ait connu au moins les deux premières de ces lois (publiées en 1609; la troisième loi, découverte le 15 mai 1618, fut publiée en 1619) cela ne paraît pas douteux. De nombreux textes pourraient en témoigner (IV : 181, 477 Li 16-22, et 481 Li 13-25; V, 259; VIII : 92, 193-5, 199-220, et 202). A ces passages de la correspondance et des œuvres du philosophe, on peut ajouter aussi les interprétations données par ses disciples et commentateurs, tel JEAN BERNOULLI, qui déduisit du système des tourbillons les raisons physiques des lois des mouvements planétaires (31).

DESCARTES reconnaît que les planètes ne décrivent presque jamais des cercles entièrement parfaits; elles s'en écartent « plus ou moins, en tout sens »; il dira seulement que le cercle de la lune « n'est pas exactement rond, mais plus long que large, en forme d'ellipse ».

(29) Anaclastique : nom général donné aux courbes apparentes dues à la réfraction.

(30) DESCARTES ne cite que deux auteurs dans sa *Dioptrique* : J. METIUS et ARCHIMÈDE. *Les Météores* aussi ne renferment que deux noms.

(31) *Nouvelles pensées sur le système de M. Descartes, et la maniere d'en déduire les Orbites & les Aphélies des Planètes*. Piece qui a remporté le prix proposé par l'Académie Royale des Sciences pour l'année 1730. Paris, 1730. BERNOULLI devait plus tard se laisser acquérir aux idées newtoniennes.

Il est remarquable qu'aussi bien dans les *Principes* que dans la correspondance notre auteur n'a fait usage du mot ellipse qu'en ce qui concerne notre satellite; il hésite visiblement à l'employer à propos des trajectoires planétaires et l'on a plutôt l'impression que, dans l'explication du mécanisme des tourbillons, ce sont des raisons d'analogie qui l'amènent à douter de la perfection des orbes circulaires des planètes. Malheureusement, le recours à la similitude est une des faiblesses des conceptions scientifiques de DESCARTES.

Les occasions où il aurait pu apprendre à mesurer toute l'importance des découvertes de KÉPLER ne manquaient pas. Rappelons par exemple qu'ISMAËL BOULLIAUD (1605-1694) avait fait la critique des deux premières lois dans sa volumineuse *Astronomia philolaica* (Paris, 1645). Or, BOUILLIAUD n'était pas un inconnu pour DESCARTES, puisque celui-ci, en mars 1638 pouvait lire, grâce à DE SAUMAISE et HUYGENS, le *De Natura lucis*, publié par BOULLIAUD en 1638 (II, 51), et le 30 octobre 1639 DE SAUMAISE écrivait à ce dernier qu'il faisait tenir son *Philolaus* au solitaire de Hollande (X, 557), à qui l'ouvrage parvint (II, 622), et qui avait d'ailleurs marqué son désir de le lire (II, 396, 464, 566 et 571).

Certes, le nom de DESCARTES ne se rattache — fort indirectement il est vrai — à l'astronomie que par ses travaux d'optique. Mais nous nous trouvons ici en présence d'un exemple qui mérite d'être plus connu, où l'on voit le grand philosophe, aveuglé par l'imagination, l'enthousiasme et un désir très vif de généralité et de science universelle, ambition qui hante alors tous les esprits, méconnaître complètement l'importance véritable de ces lois si laborieusement acquises et qui constituaient une des plus grandes découvertes de son siècle.

Ainsi, bien qu'ayant survécu de huit ans à GALILÉE, DESCARTES n'a pas montré qu'il sût, mieux que le savant italien, apprécier les travaux de KÉPLER. Son attitude en cette occasion doit être rapprochée de la sévérité qu'il témoigne à l'égard des ROBERVAL, des PASCAL et des FERMAT.

Ce qui précède suffira pour que le lecteur sente avec quelles restrictions non négligeables on peut accorder à Mr. ADAM que « notre philosophe se tenait toujours comme à l'affût des nouveautés, et ne manquait pas de les faire aussitôt servir à ses démonstrations » (XII, 390).

DESCARTES ET GALILÉE

Nous n'avons pas à nous occuper ici des conséquences, soit directes, soit indirectes mais plus profondes puisqu'elles devaient modifier à jamais la physionomie véritable de DESCARTES — du décret de 1633 (32); c'est pourquoi nous n'insisterons nullement sur l'allusion à la condamnation de GALILÉE que l'on trouve dans le *Discours de la Méthode* (VI, 60) et sur les passages de la correspondance où GALILÉE est cité, mais qui ont trait à la publication du *Monde*; nous ne considérerons que ceux où DESCARTES exprime son opinion sur le physicien florentin.

Mr. ADAM a suggéré que « le télescope avec les merveilles qu'il fit voir aussitôt dans le ciel, fut peut-être le grand évènement qui enthousiasma la jeunesse de DESCARTES, et dont toute sa philosophie devait plus tard se ressentir » (XII, 30). Il est toutefois significatif que DESCARTES ne semble pas avoir cherché à rencontrer GALILÉE lors du voyage qu'il fit en Italie en 1623-25. Quelles que soient les raisons pour lesquelles il n'est pas allé rendre visite au savant depuis longtemps fameux, il n'a plus jamais eu occasion de faire sa connaissance : « ... touchant GALILÉE, ie vous diray que ie ne l'ay iamais vû, ny n'ay eu aucune communication auec luy... » (II, 388; 11 octobre 1638).

VOLTAIRE n'avait pas été le premier à blâmer DESCARTES (33) de n'avoir jamais cité GALILÉE (II, 25-26). Il n'est pas exagéré de dire que DESCARTES ignora le grand astronome, et cependant, comme l'a rappelé PIERRE BOUTROUX, « la tradition qui a placé GALILÉE au premier rang des réformateurs de la science n'est point une de ces traditions tardives qu'établissent après coup les historiens. Elle date du vivant même du savant. Ce n'est pas LAGRANGE, ce n'est pas même NEWTON, qui a découvert en GALILÉE un initiateur; ce sont les contemporains du maître, TORRICELLI, MERSENNE, GASSENDI et les autres membres de ce petit groupe scientifique français... » (34). D'autre part, « l'homme qui a le

(32) On sait que le philosophe, effrayé par la condamnation de GALILÉE, supprima son *Monde*. Voir sur ce sujet XII, 165-179 et l'article de A. FAVARO dans *Scientia*, t. 20, p. 3-13 suppl.

(33) *Siècle de Louis XIV*, chap. 31 (des Sciences).

(34) P. BOUTROUX : Le père MERSENNE et GALILÉE, *Scientia*, t. 31, pp. 279-290 et 347-360; pp. 280-1.

plus contribué à faire pénétrer en France l'enseignement de GALILÉE est sans contredit le Père MERSENNE » (35) dont les expériences, depuis 1638, étaient directement provoquées par les *Discorsi* de GALILÉE.

Abordons les textes : « Aussy ne voy-ie rien en ses liures qui me face enuie, ny presque rien que ie voulusse auouer pour mien. Tout le meilleur est ce qu'il a de Musique; mais ceux qui me connoissent peuuent plutost croire qu'il l'a eu de moy, que moy de luy... » (II, 388-9). DESCARTES est si négligent qu'il confond ici GALILÉE (dont aucun écrit n'a trait à la musique) avec le père de l'illustre italien, VINCENT GALILÉE, mort en 1591, compositeur et auteur d'ouvrages théoriques sur la musique. Ceci est d'autant plus caractéristique que l'opinion de DESCARTES à l'égard de GALILÉE n'a jamais varié. On peut s'en convaincre en recueillant quelques appréciations dans des lettres, adressées la plupart à MERSENNE.

« Pour ce que vous me mandez du calcul que fait GALILÉE, de la vitesse que se meuuent les cors qui descendent, il ne se rapporte aucunement à ma Philosophie... » (I, 261; nov. ou déc. 1632).

« ... ie trouve qu'il philosophe assés bien du mouuement, encore qu'il n'y ait que fort peu des choses qu'il en dit, que ie trouue entierement veritable; mais, a ce que i'en ay pû remarquer, il manque plus en ce ou il suit les opinions desia receues, qu'en ce ou il s'en esloigne » (I, 304; 14 août 1634);

et dans la même lettre :

« il m'est impossible de resoudre absolument aucune question de physique qu'apprés auoir expliqué tous mes principes » (I, 305).

Les Mechaniques de Galilée de MERSENNE paraîssent en 1634. DESCARTES ne les lira qu'en 1638 : « ie puis seulement dire que ny GALILÉE, ny aucun autre ne peut rien determiner touchant cela (la chute des graves) qui soit clair & demonstratif, s'il ne sçait premierement ce que c'est que la pesanteur, & qu'il n'ait les vrais principes de la physique » (I, 392; 22 juin 1637 (?)).

La suffisance de notre philosophe revêt parfois des formes naïves : « encore que ie ne le nomme point, ie me persuade qu'il n'auroit pas méprisé ma Dioptrique » (II, 30; 1er mars 1638). Son indifférence aussi à l'égard des contemporains mérite d'être

(35) *Id.*, p. 281.

notée : « Vous m'escriuez de GALILEE comme s'il estoit encore viuant, & ie pensois qu'il fust mort il y a long tems » (III, 74; 11 juin 1640). GALILÉE ne mourut qu'en janvier 1642...

Les *Discorsi* paraissent en 1638 à Leyde; « cependant c'est de Paris que vinrent les instances auprès de DESCARTES, de les lire et d'en donner son avis : de lui-même, il ne l'aurait pas fait, et n'y pensait pas » (XII, 285). « ie ne l'ay point encore vû, dit-il à propos du dernier ouvrage de GALILÉE; mais si tost qu'il sera en vente, ie le verray, seulement afin de vous pouuoir enuoyer mon Exemplaire apostillé, s'il en vaut la peine, ou du moins vous en enuoyer mes obseruations » (II, 194; 29 juin 1638). En août 1638, DESCARTES le parcourt rapidement : « i'y trouue si peu de quoy remplir les marges, que ie croy pouuoir mettre en une fort petite lettre tout ce que i'y pourray remarquer, & ainsy que ce ne sera pas la peine que ie vous enuoye le liure » (II, 336; 23 août 1638).

Voici ce qu'on lit au début de la lettre promise (II, 380; 11 octobre 1638) :

> « Ie trouue en general qu'il philosophe beaucoup mieux que le vulgaire, en ce qu'il quitte le plus qu'il peut les erreurs de l'Eschole, & tasche a examiner les matieres physiques par des raisons mathématiques. En cela ie m'accorde entierement avec luy & ie tiens qu'il n'y a point d'autre moien pour trouuer la verité. Mais il me semble qu'il manque beaucoup en ce qu'il fait continuellement des digressions & ne s'areste point a expliquer tout a fait une matiere; ce qui monstre qu'il ne les a point examinées par ordre, & que, sans auoir consideré les premieres causes de la nature, il a seulement cherché les raisons de quelques effets particuliers, & ainsy qu'il a basti sans fondement. »

Signalons aussi cette appréciation : « sa façon d'escrire par dialogues, ou il introduit trois personnes qui ne font autre chose que louer & exalter ses inuentions chascun a son tour, aide fort a faire valoir sa marchandise » (II, 381-2). Il y a dans cette lettre une trentaine de remarques, où l'on rencontre beaucoup d'erreurs d'interprétation; DESCARTES qualifie de sophismes de longues considérations sur des sommes de vides infiniment petits où GALILÉE fait usage du langage des indivisibles. La sévérité de DESCARTES tient au fait qu'il n'a pas vu que GALILÉE considérait ces petits vides physiques beaucoup plutôt comme étant en puissance que comme étant en acte; c'est ce qu'a montré clairement TANNERY (II, 404), et ce serait une grave erreur d'en induire que

DESCARTES donne ici une preuve de son incapacité prétendue à contribuer activement au progrès des méthodes infinitésimales (36).

A deux reprises, notre philosophe spécifie pourquoi GALILÉE « a tout basti en l'air » (II, 386 et 388). Et plus tard : « Ie n'ay rien dit sur GALILÉE de ses portées de Canon qu'il reduit en tables, à cause qu'aprés auoir désaprouué toutes les raisons sur lesquelles il les fonde, il m'a semblé qu'elles ne valoient pas seulement le parler » (II, 466; décembre 1638).

Après les extraits qui précèdent, un seul exemple suffira, je pense, à indiquer les raisons véritables de la désapprobation dont GALILÉE a été continuellement l'objet. Si en statique, DESCARTES s'oppose résolument à GALILÉE en refusant de considérer la vitesse (Explication des engins... I, 435-447) (37) c'est en partie parce que la forme qu'il donne à son principe (équivalence des produits des poids par les hauteurs où ils s'élèvent) devait permettre d'arriver sans difficulté à la notion de quantité de mouvement, « notion qui à ses yeux avait la réalité et l'objectivité suprême, et dont la constance n'était rien de moins que le reflet dans le monde de la matière de l'immutabilité divine » (38), puisque aussi bien Dieu en effet est la première cause du mouvement et il en conserve toujours une égale quantité en l'univers (cf. *Principia*, pars secunda, XXXVI : VIII, 61).

Ainsi, nous sommes ramenés finalement à des raisons métaphysiques. DESCARTES a cédé une fois de plus à ses tendances naturelles. Ce qu'il reprochera surtout à GALILÉE (39) génie moins

(36) Voir G. MILHAUD, *Descartes Savant*, pp. 162-175.

(37) « Car il est impossible de rien dire de bon & de solide touchant la vitesse, sans auoir expliqué au vray ce que c'est que la pesanteur, & ensemble tout le système du monde » (II, 355). « Pour ce qu'a écrit GALILÉE touchant la balance & le leuier, il explique fort bien *quod ita sit*, mais non pas *cur ita sit*, comme ie fais par mon Principe » (II, 433)... « la raison qui fait que ie reprens ceux qui se seruent de la vitesse pour expliquer la force du leuier, & autres semblables, n'est pas que ie nie que la mesme proportion de vitesse ne s'y rencontre tousiours; mais pource que ceste vitesse ne comprend pas la raison pour laquelle la force augmente ou diminuë, comme fait la quantité de l'espace, & qu'il y a plusieurs autres choses à considerer touchant la vitesse, qui ne sont pas aysées à expliquer » (III, 614).

(38) G. MILHAUD, *Descartes Savant*, p. 180.

(39) Le malentendu avec PASCAL lors des expériences sur le vide procède des mêmes motifs. L'auteur du *Traité de l'équilibre des liqueurs* recherchait en science ceux qui ont été non des philosophes systématiques mais de purs savants : il traitait de pair avec ROBERVAL; combien son attitude dans ce cas précis a différé de celle de DESCARTES.

déductif qu'inductif, c'est que « sans auoir consideré les premieres causes de la nature, il a seulement cherché les raisons de quelques effets particuliers, & ainsy qu'il a basti sans fondement » (II, 380). De même, cette remarque encore dans la longue lettre du 11 octobre 1638, que nous avons déjà citée amplement, où il accumule pêle-mêle les critiques : « Il (GALILÉE) manque en tout ce qu'il dit de l'infini, en ce que, nonobstant qu'il confesse que l'esprit humain, estant fini, n'est pas capable de le comprendre, il ne laisse pas d'en discourir tout de mesme que s'il le comprenoit » (II, 383).

Qu'il faille trouver dans le goût inné du philosophe français pour les principes généraux et abstraits (40) les raisons profondes de son dédain à l'égard de l'illustre florentin, nous n'en douterons plus depuis que P. BOUTROUX a donné une confirmation indirecte de notre pressentiment dans les conclusions de sa brillante étude consacrée aux relations du P. MERSENNE et de GALILÉE : « il est manifeste, disait l'éminent professeur au Collège de France, que les savants, les mathématiciens, — exception faite pour les spécialistes de l'astronomie, dont MERSENNE n'était pas — admirent par dessus tout, dans l'œuvre de GALILÉE, les théories mécaniques, qui leur paraissent plus nouvelles, plus originales que tout le reste. De ces mathématiciens (ROBERVAL, FERMAT, GASSENDI, FRÉNICLE, LE TENNEUR, FLORIMOND DE BEAUNE) MERSENNE exprime sans doute exactement la pensée, car il se déclare sans cesse d'accord avec eux. Or MERSENNE, dans ses écrits proprement scientifiques, se prononce en termes très clairs et catégoriques : ce qui détermine son enthousiasme c'est l'accord que GALILÉE a su réaliser entre l'expérience et l'expression mathématique des faits mécaniques » (41).

Ajoutons que l'attitude de DESCARTES est particulièrement digne de remarque, puisque « nulle part plus qu'en France, GALILÉE n'a été admiré et suivi comme chef d'école » (42).

Ce paragraphe était écrit déjà lorsqu'a paru le travail de Mr. G. DE GIULI sur « *Galileo e Descartes* » (43).

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(40) Il doit être bien entendu que pour DESCARTES, ces principes, quelque a priori qu'ils paraissent, présupposent cependant une connaissance sensible de l'univers.

(41) P. BOUTROUX, *loc. cit.*, p. 357.

(42) *Id.*, p. 356.

(43) *Scientia*, vol. 49, pp. 207-220 (mars 1931).

L'auteur place tout d'abord la question sur le terrain de la méthodologie. Il conclut que « il n'existe pas seulement entre les deux penseurs des conformités profondes et substancielles... : il existe encore une... profonde identité entre la méthode et la philosophie de l'un et de l'autre... » C'est parce que nous avions reconnu comme évidente cette identité foncière que nous avons insisté sur des divergences que Mr. DE GIULI ne retient pas. Ce que nous avons cherché à mettre en relief, ce sont les conséquences des goûts et du tempérament de DESCARTES et de GALILÉE.

Abordant en suite la partie plus strictement philosophique des doctrines, l'auteur, un peu confusément, avouons-le, « doit reconnaître, là encore, une remarquable série de concordances entre les deux penseurs ».

J'ai été étonné de voir Mr. DE GIULI mentionner le « retour fréquent, persistant, du nom de GALILÉE dans les œuvres et surtout dans les lettres de DESCARTES ». Si nous négligeons l'allusion à la condamnation de GALILÉE dans le *Discours de la Méthode* (VI, 60), où donc Mr. DE GIULI a-t-il trouvé dans les œuvres des mentions fréquentes, persistantes du grand homme d'Arcetri ?

DESCARTES ET HARVEY

Il est certain que DESCARTES n'a jamais rencontré HARVEY ; cela ressort clairement de sa deuxième lettre (à BOSWELL ?) (1646 ?) (IV, 699-700) ; au surplus, son voyage en Angleterre vers 1631 n'est resté qu'un projet (III, 87).

Il est surprenant que le traité *de Motu Cordis* ayant été publié sur le continent (Francfort, 1628) DESCARTES ne l'ait lu seulement que quatre ans plus tard, après avoir déjà écrit le *Traité de l'Homme* (1632) (44) comme nous l'apprend une lettre de novembre ou

(44) Mais même DESCARTES eût-il connu le *De Motu Cordis* qu'il n'en aurait vraisemblablement pas fait mention toutefois, eu égard aux conditions qu'il s'imposa pour la composition du *Traité de l'Homme* : « ... pour ombrager un peu toutes ces choses, & pouuoir dire plus librement ce que i'en iugeois, sans estre obligé de suiure ny de refuter les opinions qui sont receuës entre les doctes, ie me resolu de laisser tout ce Monde icy a leurs disputes, & de parler seulement de ce qui arriuerait dans un nouueau, si Dieu creoit maintenant quelque part, dans les Espaces Imaginaires, assez de matiere pour le composer... » (VI, 42). Aucun auteur n'est cité dans ce *Traité de l'Homme*. Le langage qu'y emploie DESCARTES ne doit pas faire illusion. Notre auteur, si l'on admet qu'il poursuit

décembre 1632 à MERSENNE : « I'ay veu le liure *de motu cordis* dont vous m'auiez autrefois parlé, & me suis trouué un peu different de son opinion, quoy que ie ne l'aye vû qu'après auoir acheué d'écrire de cette matiere » (I, 263).

Dès le début — et DESCARTES conservera toujours cette attitude, bien que ne marchandant pas son admiration au savant anglais — il insiste sur le fait que ses vues diffèrent de celles de HARVEY. Dans la suite, il précisera d'ailleurs les raisons de cette divergence d'opinions.

Ici encore une fois, n'ayant de cesse qu'il ait trouvé les causes de tout, il a été conduit à donner, pour rendre compte du mouvement du cœur, des raisons insoutenables (45).

Le *Discours de la Méthode* (1637), dont la cinquième partie contient un long passage consacré à « l'explication du mouuement du cœur & de quelques autres difficultez qui appartienent à la Medecine » (VI, 1) est le premier ouvrage qui mentionne HARVEY. Les termes sont des plus flatteurs : « ... un medecin d'Angleterre (en marge : Heruaeus, *de motu cordis*), auquel il faut donner la louange d'auoir rompu la glace en cet endroit, & d'estre le premier qui a enseigné qu'il y a plusieurs petits passages aux extremitez des arteres, par où le sang qu'elles reçoiuent du cœur entre dans les petites branches des venes, d'où il se va rendre derechef vers le cœur, en sorte que son cours n'est autre chose qu'une circulation perpetuelle. Ce qu'il prouue fort bien... » (VI, 50-51). Cependant, dans sa lettre au médecin PLEMPIUS (15 février 1638) (I, 521-536), DESCARTES, répondant aux arguments de ce recteur de l'Université de Louvain qui se réclamait de GALIEN indique des points sur lesquels lui-même ne craint pas de s'écarter des idées de HARVEY : « quo experimento Haruaei sententia de motu cordis iugulatur... » (I, 527).

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un autre but que la simple vulgarisation, ne parvient pas à donner une base solide à ses brillantes intuitions. Une démonstration scientifique sérieuse de la circulation fait encore défaut : « la pluspart (des parties du sang) retournent dans les venes par les extremitez des arteres, qui se trouuent en plusieurs endroits iointes à celles des venes. Et des venes il en passe peut-estre aussi quelques parties en la nourriture de quelques membres; mais la plus-part retournent dans le cœur, puis de là vont derechef dans les arteres : en sorte que le mouuement du sang dans le corps n'est qu'une circulation perpetuelle » (XI, 127).

(45) Cf. EMIL WALBACH, *Descartes und Harvey*, *Archiv für Geschichte der Medizin*, t. 20, 301-6, 1928.

Comme je l'ai dit plus haut, DESCARTES ne laisse échapper aucune occasion de répéter qu'il ne peut se ranger à l'interprétation donnée par HARVEY : « Vous me mandez qu'un Medecin Italien a escrit contre Herueus de *motu cordis*, & que cela vous fait estre marris de ce que ie me suis engagé a escrire de cete matiere; en quoy ie vous diray franchement que ie ne vous sçaurois remercier de votre charité en mon endroit : car il faut que vous ayez bien mauuaise opinion de moy, puis que, de cela seul qu'on vous dit qu'un autre a escrit, non pas contre moy : (car, bien que ceux qui ne regardent que l'ecorse iugent que j'ay escrit le mesme qu'Herueus, a cause de la circulation du sang, qui leur donne seule dans la veue, i'explique toutefois tout ce qui appartient au mouuement du cœur d'une façon entierement contraire a la siene); mais de ce quelqu'un a escrit quelque chose, que vous imaginez estre contre moy, sans auoir oui ses raisons, ny mesme sçauoir s'il est habile homme, vous supposez incontinent que i'ay failly. Ie voy de là, & de plusieurs autres telles choses, que les bonnes raisons ont fort peu de force pour persuader la vérité, ce qui me fait presque resoudre d'oublier tout a fait a escrire, & n'estudier iamais plus que pour moy mesme » (II, 500-1);

... « neque enim hîc valet Heruaei exemplum, qui longius hinc abest quam ego, nec, ut puto, Vallaeo tam coniunctus est quam ego tibi; & quamuis esset res similis, non tam exemplo moueor quàm causâ » (à REGIUS, mai 1640; III, 66).

De 1638 à 1641, les occasions et polémiques étaient nombreuses où DESCARTES intervint à propos de la circulation.

Dans sa lettre du 5 juillet 1643 à BEVERWICK, il explique longuement une fois de plus les raisons pour lesquelles il est d'un sentiment tout-à-fait contraire à HARVEY en ce qui concerne le mouvement du cœur, bien que, dit-il « circa sanguinis circulationem cum Heruaeo plane consentiam, ipsumque ut praestantissimi illius inuenti, quo nullum maius & utilius in medicinâ esse puto, primum auctorem suspiciam... » (IV, 4).

Enfin, le passage suivant d'une lettre adressée en 1646 peut-être à BOSWELL, ambassadeur d'Angleterre à La Haye, montre, comme on l'a vu ci-dessus, que DESCARTES n'a pas connu personnellement HARVEY, et résume très exactement son attitude à l'égard de l'illustre Anglais :

« An non nosti Londini celebrem quendam medicum nomine Hervaeum, qui

librum de motu cordis & circulatione sanguinis conscripsit? quis homo est? Equidem de motu cordis nihil dicit, quod in alijs iam non extaret, neque illi per omnia assentior; sed quantum ad circulationem sanguinis, triumphat, ipsique honor debetur quod fuerit primus inuentor, in quo Medicina ei multum debet. Is promittebat alios quosdam tractatus, sed nescio an quippiam postea ediderit; talia enim opuscula magis digna sunt quae lucem aspiciant, quam magnus numerus crassorum voluminum, quibus charta inutiliter commaculatur » (IV, 699-700).

Dans *La Description du corps humain*, traité inachevé composé en 1648, DESCARTES mêle une dernière fois les éloges aux critiques (XI, 239-243) et donne une preuve nouvelle de l'impatience de son imagination en concluant que tout « cela ne prouue autre chose, sinon que les experiences mesme nous donnent souuent occasion de nous tromper, lors que nous n'examinons pas assez toutes les causes qu'elles peuuent auoir » (XI, 242).

Il convient d'ajouter qu'il n'est pas douteux que DESCARTES, en dépit de ses réserves, a contribué dans une très notable mesure à consolider la position de HARVEY (46).

KÉPLER (*)

KÉPLER ET BACON

BACON n'a jamais été cité par l'astronome allemand.

KÉPLER ET DESCARTES

DESCARTES ne publia son premier ouvrage (*Discours de la Méthode*, 1637) que sept ans seulement après la mort de KÉPLER. On ne peut donc s'attendre à trouver des références le concernant chez le mathématicien de l'Empereur.

KÉPLER ET GILBERT

KÉPLER s'occupait du magnétisme dès 1599, avant la publication

(46) Il y a des références sans intérêt à HARVEY dans la *Lettre à Voet* (mai 1643), écrit polémique (VIII, 120), dans la première lettre au marquis de Newcastle (1645; IV, 189) et dans *Les Passions de l'âme* (1649; XI, 332).

(*) Pour l'étude de KÉPLER, j'ai utilisé l'édition FRISCH : *J. Kepleri opera omnia*, 8 vol., Francfort, 1858-1871. Sauf mention contraire, toutes les citations dans ce chapitre se rapportent à cette édition, les chiffres romains indiquant le volume, les chiffres arabes la page.

du *De Magnete* (1600), et même lorsqu'il eût reconnu l'impossibilité de déterminer la position des pôles magnétiques d'après la mesure de la déclinaison, il continua de s'intéresser à la question, car, n'accordant aucune place à la gravitation dans sa mécanique céleste, il croyait (notamment après 1609, année de la découverte des deux premières lois des mouvements planétaires) que l'intensité du magnétisme des planètes pouvait rendre raison de leurs orbites elliptiques. Ceci suffit à montrer l'importance qu'eut pour lui le traité *De Magnete* où l'auteur prouvait la possibilité d'assimiler la terre à un aimant (47). Ne nous étonnons donc pas que KÉPLER ait consacré un long paragraphe de son *Epitome Astronomiae Copernicanae* (quatrième livre, 1620) à l'exposition et à la défense de la théorie de GILBERT (48).

Il est inutile de reproduire ici tous les passages des ouvrages de KÉPLER où le physicien anglais est nommé et où ses contributions et ses opinions sont discutées. Dès le début l'attitude de KÉPLER fut extrêmement favorable : « Mihi sane nihil falsum Copernicus dixisse videtur : quodque meis pro Copernico defuit argumentis, id admirabili solertia experiundique industria supplesse videtur Gilbertus Guilielmus Anglus in re magnetica » lit-on dans un fragment (I, 243) probablement postérieur de peu à l'année 1600. En 1608, HARRIOT fait part à KÉPLER (II, 74) du plaisir qu'il a éprouvé à lire dans *Ad Vitellionem Paralipomena* (1604) l'éloge suivant de son compatriote : « Vir equidem talis, cujus divinis inventis omnes naturae studiosos plurimum delectari convenit. Cujus familiaritatem, nisi mihi pomposa illa Tethys invideret,

(47) « Quid si ergo corpora planetarum omnia sunt ingentes quidam rotundi magnetes? De Terra (uno ex planetis, Copernico) non est dubium. Probavit id Guilelmus Gilbertus » (De Motibus Stellae Martis, 1609; III, 387); dans le même ouvrage on lit encore : « Perbellum equidem attigi exemplum magnetis et omnino rei conveniens, ac parum abest quin res ipsa dici possit. Nam quid ego de magnete, tanquam de exemplo? cum ipsa Tellus, Gulielmo Gilberto Anglo demonstrante, magnus quidam sit magnes, eademque eodem auctore, Copernici assertore, convolvatur in dies singulos, uti ego Solem volvi conjicio... Itaque plausibile est, cum Terra Lunam cieat per speciem, sitque corpus magneticum, et Sol planetas cieat similiter per emissam speciem : Solem itaque similiter corpus esse magneticum » (III, 307).

(48) « *Incredibile vero est, corpora coelestia esse quosdam ingentes magnetes.* Legatur ergo Gulielmi Gilberti Angli philosophia magnetica, quo libro, quamvis non crederet auctor, Terram inter sidera ferri, tribuit illi tamen naturam magneticam argumentis bene multis... » (VI, 375).

discendi ardore me demereri sine magna difficultate posse, ut non est superba philosophia, speraverim » (II, 221) (49), et dans une longue lettre à HERWART de Hohenburg, KÉPLER écrivait le 12 janvier 1603 :

« Verum ita fuse, distincte, sufficienter tractavit hanc rem Gilbertus, ita undique nobilissimis experimentis se munivit, omnium contradicentium ora obstruxit, ut plane evanescant meae speculationes de tarda illa migratione poli motus diurni ex locis Terrae, quae in creatione illi fuerint subjecta » (III, 445).

Il fait intervenir Gilbert dans les discussions les plus variées :

« Stellis praeest virtus aliqua, geometriae capax globi, potens ad motum, tempus numerans. Ego, si, quod jubes, scapham scapham appellare debeo, non possum aliter hanc, quam intelligentiam, vel mentem appellare. Si aliud tibi nomen praeplacuerit, dicito. Interim hoc manet discrimen, quod ratiocinatione non utuntur, hoc bono Guil. Gilberti illius argumento, quod ratiocinationem sequatur in actione inconstantia, inaequalitas et titubatio, apud homines quidem » (I, 319);

ce n'est pas seulement pour ses travaux sur le magnétisme que le médecin anglais est apprécié : comme il arrive presque toujours, l'opinion du savant est invoquée dans des questions étrangères à ses recherches :

« Itaque defendit illam (fixarum sphaeram infinitam) infelix ille Jordanus Brunus : nec obscure asseruit specie dubitantis et Guilielmus Gilbertus libro de Magnete, cetera praeclarissimo religiosum tamen affectum eo demonstravit, quod existimaret, non alia re rectius intelligi infinitam Dei potentiam, quam si infinitum mole conderet mundum » (II, 688; voir aussi sur ce sujet : VI, 169).

KÉPLER le met donc sans hésitation au rang des plus grands :

« Veritatis in me sit amor an gloriae, loquantur dogmata mea, quae pleraque aliis accepta fero : totam astronomiam Copernici hypothesibus de mundo, Tychonis vero Brahei observationibus, denique Gulielmi Gilberti Angli magneticae philosophiae inaedifico » (VI, 306* : 1620).

KÉPLER ET GALILÉE

Nous n'entrerons pas dans de nombreux détails en ce qui concerne les relations si importantes de KÉPLER et de GALILÉE, car récemment une étude assez complète a paru sur ce sujet (50).

(49) Il résulte de ce passage que KÉPLER n'avait pas encore eu connaissance de la mort de GILBERT (1603).

(50) HOPPE, Kepler und Galilei; *Archiv für Geschichte der Mathematik, der Naturwissenschaften und der Technik*, t. 11, 363-385; cf. aussi : E. WOHLWILL, *Galilei*, 2 vol., 1909 et 1926.

Il nous semble intéressant de noter ici l'unique cas d'une correspondance entre deux des savants qui font l'objet du présent travail. Rappelons que cinq d'entre eux étaient pourtant contemporains. Il y a là une curieuse illustration de la situation caractéristique d'une époque où l'isolement était chose normale, des personnages parfois fort obscurs servant d'agents de liaison; situation d'ailleurs qui, en dépit de la fondation des académies, se prolongea jusqu'à la fin du siècle : NEWTON par exemple eut recours à des intermédiaires et ne rencontra qu'une seule fois un savant étranger de marque, HUYGENS.

On connaît sept lettres de KÉPLER à GALILÉE et trois de GALILÉE à KÉPLER. Tous ces messages sont en latin. C'est l'astronome italien qui le premier prend la parole, le 4 août 1597, de Padoue (51) pour remercier en termes chaleureux son collègue de l'envoi du *Prodromus* (1596) :

« Miserabile enim est, dit-il notamment (p. 68), adeo raros esse veritatis studiosos, et qui non perversam philosophandi rationem prosequantur. At quia non deplorandi nostri saeculi miserias hic locus est, sed tecum congratulandi de pulcherrimis in veritatis confirmationem inventis, ideo hoc tantum addam, et pollicebor me aequo animo librum tuum perlecturum esse, cum certus sim me pulcherrima in ipso esse reperturum. Id autem eo libentius faciam, quod in Copernici sententiam multis abhinc annis venerim, ac ex tali positione multorum etiam naturalium effectuum caussae sint a me adinventae, quae dubio procul per comunem hypothesim inexplicabiles sunt. Multas conscripsi et rationes et argumentorum in contrarium eversiones, quas tamen in lucem hucusque proferre non sum ausus, fortuna ipsius Copernici, praeceptoris nostri, perterritus, qui, licet sibi apud aliquos immortalem famam paraverit, apud infinitos tamen (tantus enim est stultorum numerus) ridendus et explodendus prodiit. Auderem profecto meas cogitationes promere, si plures, qualis tu es, exstarent : at cum non sint, huiusmodi negotio supersedebo.

Temporis angustia et studio librum tuum legendi vexor : quare huic finem imponens, tui me amantissimum atque in omnibus pro tuo servitio paratissimum exibeo. Vale, et ad me iucundissimas tuas mittere ne graveris ».

En dépit des conseils empressés et des encouragements prodigués par KÉPLER dans sa réponse (13 octobre 1597) (52) GALILÉE ne reprit la plume pour son correspondant allemand que le 19 août 1610 (53).

KÉPLER, tout à l'ardeur de son enthousiasme pour les découvertes

(51) *Le Opere di Galileo Galilei*, edizione nazionale, 1890-1909, t. 10, pp. 67-68.
(52) *Id.*, t. 10, pp. 69-71.
(53) *Id.*, t. 10, pp. 421-3.

astronomiques de son confrère italien, avait fait imprimer à ses frais à Prague le *Sidereus Nuncius* dont GALILÉE lui avait fait tenir un des premiers exemplaires (édition de Venise, mars 1610) mais avec, en outre, en guise de préface une longue lettre à GALILÉE lui-même (19 avril 1610) intitulée *Dissertatio cum nuncio sidereo* (FRISCH, II, 485-506). GALILÉE, très satisfait, bien qu'un passage lui ait déplu, reproduisit cette préface dans une édition publiée à Florence peu après. KÉPLER se plaignit du procédé, quoique l'auteur du *Sidereus Nuncius* eût été en droit également de manifester quelque mécontentement; en réalité tout porte à croire que KÉPLER feignit seulement d'être offensé, car il termine sur une boutade :

« Dissertationem edidi meis sumptibus, misique Francofortum aliquem iustum numerum. Florentinus itaque typographus ad damnum me redigit sua editione : id per se inhumanum; an etiam inuistum, viderit Florentia. Nam si non recognoscit Caesarem superiorem, nihil queror; sin autem, equidem privilegio munitus erat libellus. Propter hanc ambiguitatem, in suspenso erit quo nomine Illustrissimo Oratori sim obligatus. At, nisi fallor, non sedet is Pragae typographi causa, sed Magni Ducis; suamque munificentiam sibi vindicat. Quod si mihi iuris aliquid esset in typographum, condemnarem illum ad multam hanc, ut tuis operis solveret pro uno bono et lato vitro convexo, quod esset fragmentum sphaerae duodecim pedum semidiametri, aut ei aequipolleret » (54) (Décembre 1610).

C'est à propos de la *Dissertatio* que KÉPLER mandait à MAGINI en cette même année 1610 : « Copernicani sumus uterque (GALILÉE et KÉPLER) : similes simili gaudet. Tuto tamen (si legas attente), me satis mihi cavisse, et ubi potui, ad sua ipsum principia revocasse ». (55)

Je n'insiste pas sur les lettres échangées dans l'intervalle, puis au début de l'année 1611 (N. S.) Elles montrent de la part de KÉPLER un attachement et une admiration sans réserves; l'on ne compte dans la suite qu'un court billet isolé écrit par GALILÉE, seize ans plus tard (28 août 1627; *Le Opere di G. G.*, t. 13, pp. 374-5). Nous verrons plus loin que GALILÉE montra dans ses Dialogues, peu après la mort de KÉPLER, qu'il n'adhérait pas entièrement à toutes les opinions de l'astronome allemand.

Je n'ai pas à décrire ici les efforts de ce dernier pour déchiffrer les logogriphes de GALILÉE concernant l'anneau de Saturne et

(54) *Id.*, t. 10, p. 507.
(55) *Id.*, t. 10, p. 353.

les phases de Vénus (56). On trouvera le récit de ses peines infructueuses dans les histoires de l'astronomie (57). Disons enfin qu'il n'est presque pas un des ses ouvrages qui ne contienne des références à GALILÉE. Il faut ajouter aussi à l'honneur de KÉPLER qu'il ne prêta l'oreille ni aux railleries de MAESTLIN à l'endroit de GALILÉE ni aux accusations de plagiat formulées contre le Florentin, probablement dans un but intéressé, par l'anglais BRUCE en 1602 et 1603 (58).

KÉPLER ET HARVEY

KÉPLER n'a jamais mentionné HARVEY.

GALILÉE (*)

GALILÉE ET BACON

L'influence de BACON sur les savants a été trop négligeable pour que l'on puisse s'attendre à ce que GALILÉE prêtât la moindre attention aux écrits du Chancelier; il ne faut donc pas être surpris que les œuvres du grand Italien ne contiennent aucune trace de son nom (59).

GALILÉE ET DESCARTES

J'ai déjà mentionné, à la fin du paragraphe relatif à l'influence de GALILÉE sur DESCARTES, le travail récent de Mr. DE GIULI. L'absence de toute référence à DESCARTES dans l'œuvre de GALILÉE ne doit guère nous étonner puisque ce dernier appartenait à une génération précédente.

En 1637, MERSENNE invita l'illustre victime d'URBAIN VIII

(56) Voir la *Narratio de Iovis satellitibus* (1611), encore un hommage à GALILÉE.

(57) Voir la lettre de KÉPLER à GALILÉE, 9 janvier 1611 : *Le Opere di G. G.*, t. 11, p. 15-17 et aussi FRISCH, II, 462 et sq.

(58) Cf. FRISCH, II, 568 et *Le Opere di G. G.*, t. 10, pp. 90 et 104.

(*) Pour l'étude de GALILÉE, j'ai fait usage de l'édition nationale italienne : *Le Opere di Galileo Galilei*, 20 vol., Florence, 1890-1909.

(59) Le témoignage de TOBIA MATTHEW (lettre à BACON, 14 avril 1619, *Le Opere di G. G.*, t. 12, p. 450) suivant lequel GALILÉE aurait répondu au *De fluxu et refluxu Maris* de BACON (écrit vraisemblablement avant 1616) reste fort sujet à caution.

à faire connaître son opinion sur le *Discours de la Méthode* et sollicita ses critiques (60), mais étant donnée la position où se trouvait GALILÉE depuis sa condamnation, on comprendra qu'il ne se soit jamais exécuté, du moins à ce qu'il semble.

GALILÉE ET GILBERT

L'influence de GILBERT sur KÉPLER et GALILÉE a été considérée dans le mémoire déjà cité de EDM. HOPPE : *Kepler und Galilei* (61) et son influence sur GALILÉE a été étudiée par S. MAGRINI (62).

Je me bornerai ici à dire qu'à la suite des travaux de GILBERT, le magnétisme se voyait intimement lié aux théories cosmogoniques. Le physicien anglais avait en Italie des partisans enthousiastes, disciples de GALILÉE mais aussi des détracteurs acharnés qui se recrutaient parmi les adversaires du grand Florentin.

GALILÉE possédait un exemplaire du *De Magnete* (63). Si l'on néglige trois références à GILBERT dans la correspondance (64) c'est dans les *Dialogi* (1632) seulement, à la fin de la troisième journée, que GILBERT a été nommé. L'œuvre de GILBERT y est longuement commentée, mais il suffira de reproduire l'éloge suivant, qui donne une idée exacte de la discussion :

« Io sommamente laudo ammiro ed invidio questo autore, per essergli caduto in mente concetto tanto stupendo circa a cosa maneggiata da infiniti ingegni sublimi, nè da alcuno avvertita ; parmi anco degno di grandissima laude per le molte nuove e vere osservazioni fatte da lui, in vergogna di tanti autori mendaci e vani, che scrivono non sol quel che sanno, ma tutto quello che senton dire dal vulgo sciocco, senza cercare di assicurarsene con esperienza, forse per non diminuire i lor libri : quello che avrei desiderato nel Gilberti, è che fusse stato un poco maggior matematico, ed in particolare ben fondato nella geometria, la pratica della quale l'avrebbe reso men risoluto nell'accettare per concludenti dimostrazioni quelle ragioni ch'ei produce per vere cause delle vere conclusioni da sè osservate » (65).

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(60) Lettre du 27 novembre 1637 : *Le Opere di G. G.*, t. 17, p. 226.

(61) *Archiv für Gesch. der Math.*, t. 11, pp. 363-385 ; voir spécialement, p. 368.

(62) Il « *De Magnete* » del GILBERT e i primordi della magnetologia in Italia in rapporto alla lotta intorno ai massimi sistemi ; *Archivio di storia della scienza*, t. 8, pp. 17-39.

(63) *Le opere di G. G.*, t. 8, p. 625.

(64) *Id.*, t. 5, p. 352, t. 13, p. 328 et t. 14, p. 36.

(65) *Le Opere di G. G.*, t. 7, p. 432.

GALILÉE ET KÉPLER

J'ai rappelé plus haut le commerce épistolaire des deux savants. Revenant tout d'abord à l'article de HOPPE, je commencerai par mentionner le fait qui semblera incroyable à beaucoup mais qui s'impose cependant avec la plus grande évidence après une lecture attentive des textes : on ne peut assurer d'une façon définitive que GALILÉE ait connu les trois lois du mouvement des planètes découvertes par KÉPLER. Rien ne prouve en tout cas qu'elles l'aient frappé.

Disons immédiatement que les explications de cette étonnante constatation tentées par HOPPE ne nous paraissent pas extrêmement satisfaisantes. Je ne pense pas qu'isolées ou prises ensemble elles puissent fournir tous les éléments nécessaires à la résolution du problème qui se pose ; elles méritent toutefois un soigneux examen.

Que GALILÉE ait pensé que ses propres découvertes constituaient des preuves plus convaincantes de la validité du système de COPERNIC que celles données par KÉPLER, c'est ce qu'il faut admettre avec KÉPLER lui-même (66). On ne saurait cependant assez souligner le caractère contingent de ses découvertes ; et d'autre part, il est peu concevable que, tout asservi et timoré qu'il ait été, il ait accordé la préférence à ses observations plutôt qu'à des preuves moins faciles à saisir peut-être, mais autrement profondes et significatives en revanche, puisqu'elles amélioraient le système de COPERNIC dans son essence même en entraînant la suppression des excentriques et des épicycles, derniers vestiges d'explications désormais ruinées.

GALILÉE connaissait certainement l'*Astronomia nova* (1609) où KÉPLER énonce ses deux premières lois, mais il est notoire que l'illustre Italien, comme DESCARTES, ne lisait guère. VIVIANI nous assure que son maître avait peu de livres. Il ne faut pas s'étonner

(66) « Ibidem negat Galilaeus, Ptolemaicam hypothesin refutari potuisse a Tychone, Copernico vel aliis ; a se refutatam esse usu telescopii in observatione variationis discorum Martis et Veneris » (FRISCH, VII, 273). Dans une lettre de 1632, GALILÉE écrivait : « ... ma per quel poco che ho potuto cosi alla spezzata comprendere, dubito che i pensieri del Landsbergio e alcuni del Keplero siano più tosto a diminutione della dottrina del Copernico che a stabilimento, parendomi che questi (come si suol dire) ne habbiano voluto troppo ». (*Li Opere di G. G.*, t. 14, 340-341).

dès lors que le style fatiguant de KÉPLER, ses obscurités, sa présentation touffue, et par dessus tout l'archaïsme de ses conceptions encore directement inspirées par l'idéal hellène de la science aient rebuté GALILÉE et l'aient empêché de poursuivre heureusement, à travers le dédale et le fouillis d'un exposé sans grâce et mal composé, les formes brillantes qui s'y trouvaient cachées (67). D'ailleurs, il convient d'insister plus que ne l'a fait HOPPE sur ce qu'on pourrait appeler l'incuriosité de GALILÉE.

G. MILHAUD se demandait « en lisant GALILÉE, qui pourtant a fait tant d'efforts pour perfectionner la lunette primitive, s'il a jamais connu les travaux d'optique de KÉPLER » (68). Or, le récit d'un entretien du Français JEAN TARDE avec GALILÉE, en novembre 1614 (69), nous fournit une indication d'autant plus précieuse sur ce point que l'on ne voit pas de raison sérieuse de révoquer ce témoignage en doute :

« Je l'interpellay (Galilée) sur les refractions & moyens de former le crystal du telescope en telle sorte que les objets s'agrandissent & s'approchent à telle proportion qu'on veut. A cela il me respondit que ceste science n'estoit pas encore bien cogneue; qu'il ne sçavoit pas que personne l'eût traictée aultre que ceux qui traictent la perspective, si ce n'est Joannes Keplerus, mathematicien de l'Empereur, qui en a faict un livre exprés, mais si obscur qu'il semble que l'autheur mesme ne s'est pas entendu » (70).

De même, nulle part GALILÉE n'a parlé de la réfraction astronomique en termes tels que l'on puisse croire qu'il ait été au courant de ce qu'en a écrit KÉPLER; il avait pourtant été averti de la publication de la *Dioptrice* par son auteur lui-même : « Scripsi Dioptricen, quae superiori Septembri venit in manus Ser.^{mi} Electoris. Puto nihil a me praeteritum, quod non ex suis causis demonstravevim. Equidem campus est exercendi ingenii : prodeat qui ex aliis demonstret principiis, quam quibus ego sum usus ». (71) N'est-il pas surprenant aussi que dans les calculs des *Dialogo*

(67) Il est piquant de noter qu'un passage de la seconde journée des *Dialogo* (1632) est intitulé : « Argomento del Keplero a favor del Copernico » (*Le Opere di G. G.*, t. 7, p. 294).

(68) G. MILHAUD, *Descartes Savant*, p. 99.

(69) Rapporté par ADAM, *Œuvres de Descartes*, t. 12, 1910, p. 67.

(70) En 1610, Galilée n'avait pas encore lu les « Ad Vitellionem paralipomena, 1604 »; cela résulte de sa lettre du 1er octobre 1610 à Giuliano de Medici (*Le Opere di G. G.*, t. 10, p. 441).

(71) KÉPLER à GALILÉE, 9 janvier 1611; *Le Opere di G. G.*, t. 11, pp. 16-17

(1632) où il cite KÉPLER à plusieurs reprises, GALILÉE n'ait fait aucun usage des logarithmes de NÉPER, de KÉPLER et de BRIGGS inventés déjà depuis longtemps ? Pour être justes, ajoutons qu'assez curieusement, la découverte de NÉPER n'excita pendant longtemps aucun intérêt en France.

En réalité, différents indices relevés dans les *Dialogues* m'ont amené à croire que GALILÉE a franchement ignoré les lois de KÉPLER, alors que DESCARTES les a connues mais leur a réservé le sort signalé plus haut. On perd de vue qu'au surplus personne ne paraît avoir fait attention à ces lois lors de leur publication (72). Il conviendrait, dès lors, de rattacher la recherche de la solution de ce troublant mystère : l'ignorance montrée par GALILÉE d'une des plus grandes conquêtes de l'astronomie, à l'étude plus générale du destin des lois de KÉPLER depuis le moment de leur publication (1609 et 1619) jusqu'à NEWTON (73).

C'est là un travail qui n'a pas été entrepris encore et dont la nécessité, qui n'a pas toujours été clairement soupçonnée semble-t-il, se fait sentir davantage au fur et à mesure que l'on approfondit l'étude de la pensée scientifique au dix-septième siècle.

En résumé, les contributions de HOPPE à la question n'ont pas forcé notre conviction ; il n'a fait que nous enrichir d'incertitudes nouvelles, mais c'est là d'un véritable historien.

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(72) La trajectoire elliptique est mentionnée, avec le nom de KÉPLER, dans une lettre de CÉSI, du 21 juillet 1612, mais il n'est pas certain que GALILÉE ait été le destinataire de ce message (*Le Opere di G. G.*, t. 11, pp. 365-7). Il existe un ensemble de faits bien curieux concernant l'histoire des lois de KÉPLER et qui attendent d'être étudiés systématiquement. Pour quelles raisons, par exemple, J. D. CASSINI (mort en 1752) semble-t-il avoir ignoré non seulement COPERNIC, mais KÉPLER et même NEWTON ?

(73) Il n'est pas moins remarquable que les ouvrages de KÉPLER ne sont mentionnés ni dans les avertissements ni dans les décrets de Rome : ses lois ont passé inaperçues alors que le système de COPERNIC constituait cependant l'enjeu de la bataille ; il a fallu la démonstration que donna NEWTON de ces lois pour en faire sentir la liaison et l'importance. HOOKE, en 1665, (*Micrographia*, p. 238) s'inquiète encore de la nature réelle de l'orbite de la lune. Dans sa note sur : « Galileo e le Leggi del Keplero » (*Atti e memorie della* R. *Accademia di scienze lettere ed arti in Padova*, anno CCCLXXV, 1915-16, nuova serie, vol. XXXII, pp. 129-132) A. FAVARO a eu l'occasion de dire qu'on ne sait même pas si la découverte du célèbre astronome relative au mouvement elliptique avait beaucoup frappé DESCARTES. Je crois devoir mentionner cette opinion car, comme on l'a vu plus haut, je suis arrivé sur ce sujet à une conclusion différente.

Il est sans intérêt de narrer ici les détails de la controverse KÉPLER-CHIARAMONTI à laquelle fût mêlé GALILÉE (1625-6). Nous préférons placer sous les yeux du lecteur deux citations qui vont nous permettre en quelque sorte de faire le point. Discutant tout à la fin de la quatrième journée des *Dialogo* (1632) la théorie des marées de SELEUCUS, GALILÉE rejette comme cause du phénomène la combinaison des mouvements de la Terre et de la lune et s'étonne que KÉPLER ait accordé créance à de telles fantaisies:

« Ma tra tutti gli uomini grandi che sopra tal mirabile effetto di natura hanno filosofato, più mi meraviglio del Keplero che di altri, il quale, d'ingegno libero ed acuto, e che aveva in mano i moti attribuiti alla Terra, abbia poi dato orecchio ed assenso a predominii della Luna sopra l'acqua, ed a proprietà occulte, e simili fanciullezze » (74).

D'autre part, dans une lettre de novembre 1634 (à FULGENZIO MICANZIO?) GALILÉE s'exprime ainsi :

« ... potrà con occasione fare intendere che io ho stimato sempre il Keplero per ingegno libero (e forse troppo) e sottile, ma che il mio filosofare è diversissimo dal suo, e che può essere che scrivendo delle medesime materie, solamente però circa i movimenti celesti, habbiamo talvolta incontrato in qualche concetto simile, se ben pochi, onde habbiamo assegnato di alcuno effetto vero la medesima ragion vera; ma questo non si verificherà di uno per cento dei miei pensieri » (75).

Il résulte de là que si GALILÉE a manifesté le plus vif respect pour le génie de KÉPLER, il a néanmoins considéré ses propres conceptions comme presque totalement affranchies de la pensée de l'astronome allemand dans le seul domaine scientifique qui fût commun à leurs travaux : l'astronomie (76) et cela, bien que ses ouvrages astronomiques les plus importants fussent postérieurs au *Prodromus* (1596) et à l'*Astronomia Nova* (1609). Il est donc permis d'affirmer que leurs deux philosophies sont en fin de compte nettement indépendantes. C'est à l'analyse de cette différence profonde entre les deux tendances que nous venons de constater qu'est consacrée en grande partie l'étude de HOPPE, et je ne puis mieux faire que d'y renvoyer le lecteur.

(74) *Le Opere di G. G.*, t. 7, p. 486.
(75) *Id.*, t. 16, p. 163.
(76) On ne peut dire : la mécanique céleste, puisque les contributions de GALILÉE à la théorie des planètes sont négligeables.

GALILÉE ET HARVEY

Voir ci-dessous le paragraphe HARVEY et GALILÉE.

HARVEY

HARVEY ET BACON

S'il faut en croire AUBREY (77), HARVEY se serait exprimé sur le compte de BACON de la façon suivante : « He writes philosophy like a Lord Chancelor » said he (HARVEY) to me, speaking in derision; « I have cured him ». Quelle que soit la valeur qu'il faille attribuer à ce témoignage, le fait est que HARVEY n'a mentionné BACON qu'une seule fois, pour lui emprunter une métaphore (78). L'argumentation de Mr. HALE-WHITE dont nous avons déjà mentionné l'étude (79) n'est rien moins que convaincante et définitive, et ne nous empêchera pas d'être d'avis que LIEBIG avait pleinement raison quand il s'exprimait, un peu lourdement il est vrai, comme suit : « Croire que de belles phrases modifient le travail et les tendances de l'humanité, c'est vouloir apaiser la faim par des tableaux figurant du pain ou de la viande; on admire peut-être le tableau s'il est bien fait, mais on n'y mord point » (80). D'ailleurs, Mr. HALE-WHITE, avoue ingénument à propos de BACON que : « ... although he taught the use of induction and experiment, he was so constituted as to have no aptitude whatever for experimentation » (81).

GILBERT s'était passé de BACON. Pourquoi n'en aurait-il pas été de même de HARVEY ? L'histoire des sciences nous montre que des esprits originaux ont pu, à une époque déterminée, travailler en toute indépendance et aborder cependant de la même manière le problème scientifique.

HARVEY ET DESCARTES

HARVEY (82) à la fin de *A second disquisition to John Riolan*,

(77) *Brief Lives*, édit. A. CLARK, Oxford, 1898; I, 299.
(78) *The Works of William Harvey*, édition R. WILLIS, Londres, 1847; p. 270.
(79) W. HALE-WHITE, *Bacon, Gilbert and Harvey*, London, 1927.
(80) LIEBIG, *loc. cit.*, p. 199.
(81) HALE-WHITE, *loc. cit.*, p. 35.
(82) *The works of William Harvey*, pp. 139 et 140.

Jun. ayant été amené à citer « l'habile et pénétrant » DESCARTES à propos des recherches de ce dernier sur le cœur des poissons, lui exprime publiquement sa reconnaissance d'avoir été favorablement mentionné dans les œuvres du philosophe (voir plus haut le paragraphe : DESCARTES et HARVEY).

HARVEY ET GILBERT

HARVEY avait vingt-cinq ans seulement à la mort de GILBERT (1603). Il est difficile de dire dans quelle mesure il fut influencé par son compatriote; au surplus, il n'est pas certain qu'il l'ait jamais rencontré. Il ne l'a pas cité.

HARVEY ET KÉPLER

Il n'y a aucune référence à KÉPLER chez HARVEY.

HARVEY ET GALILÉE

On ne saurait assez exagérer l'importance qu'aurait pour l'histoire de la pensée scientifique et des influences la découverte de documents relatifs à d'éventuelles relations entre GALILÉE et HARVEY durant leur séjour simultané à Padoue (83). L'absence complète de toute information sur ce sujet (on ne trouve notamment aucune référence à GALILÉE chez HARVEY, et réciproquement) ne justifie guère le silence que les auteurs ont gardé jusqu'à présent sur cette question. Il est possible que le jeune anglais ait entendu et connu GALILÉE, mais il ne faudrait pas s'étonner cependant qu'on ne découvre jamais la moindre trace de leurs relations; il est en effet tout naturel que, bien que peut-être très fortement influencé par un savant de génie — qui avait, il est intéressant de le rappeler ici, débuté dans la carrière scientifique par la médecine, — HARVEY n'ait pas eu l'occasion ou éprouvé le besoin de mentionner ce qu'il devait à l'illustre maître de Padoue.

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(83) GALILÉE professa dans cette ville pendant dix-huit ans, depuis la fin de 1592 jusqu'en 1610; HARVEY étudia à l'Université de Padoue durant cinq années, de 1597 (il avait alors dix-neuf ans) à 1602.

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CONCLUSIONS

Nous nous bornerons à quelques remarques générales, qui n'ont pu trouver place ailleurs.

Au nombre des matières obscures ou difficiles qui se sont présentées au cours de nos recherches, nous avons souligné l'importance particulière de deux questions qui méritent de retenir l'attention de l'historien des sciences et sur lesquelles la lumière est loin d'avoir été faite encore. L'une a trait à l'histoire mal connue des lois de KÉPLER et à leur étrange destin depuis leur publication jusqu'au moment où NEWTON, grâce à la loi de gravitation, en montra la liaison. L'impossibilité d'obtenir une détermination géométrique de l'anomalie vraie n'est pas suffisante pour justifier l'étonnant accueil qu'elles reçurent.

L'autre question, sur laquelle les documents font totalement défaut, concerne la possibilité de relations entre GALILÉE et HARVEY durant les cinq années de leur commun séjour à Padoue.

Nous avons constaté que les deux philosophes, BACON et DESCARTES, n'ont que peu cité, très différents en cela des savants que rien n'empêchait de nommer les auteurs auxquels ils étaient redevables d'idées ou de données : KÉPLER citait ses sources sans se lasser. L'honnêteté scientifique, au sens où nous l'entendons aujourd'hui, était plus grande chez les savants purs.

D'autre part, nous avons noté le danger des conceptions abstraites ou métaphysiques appliquées aux résultats de l'expérience lorsqu'elles sont inspirées par le désir trop vif de connaître les causes des phénomènes et qu'à ce désir se mêlent des explications suggérées par la théologie. Parfois, sous la pression de telles doctrines, le savant a pu méconnaître complètement les idées d'un contemporain, même lorsque tous deux appliquent les ressources de leur génie à un domaine identique (jugement de GALILÉE sur KÉPLER). Il faut dire qu'en revanche, des idées très abstraites et peu tangibles comme les lois de KÉPLER, bien plus aisées à dégager par le raisonnement qu'à surprendre dans la réalité, ont contre elles de s'imposer avec plus de difficulté que les simples constatations par lesquelles GALILÉE fournit la preuve de la validité du système de COPERNIC.

Nous avons vu que l'attitude de DESCARTES vis-à-vis de ses contemporains les plus fameux est dans certains cas justifiée, par la prudence par exemple en ce qui concerne les travaux astronomiques de GALILÉE (84). Cependant si on note qu'il n'a cité dans son œuvre ni KÉPLER ni GALILÉE, la remarque de Mr. ADAM suivant laquelle « DESCARTES n'admirait presque rien ni personne » (XII, 285) reprend toute sa valeur. Par tempérament, DESCARTES ne lisait pas volontiers et lisait très vite. Le fait qu'il se rendit en Hollande moins dans la crainte de persécutions (85) que pour y philosopher plus à l'aise, par soif de repos et horreur des polémiques, est caractéristique (86). Certes, le philosophe français ne fut ni courageux ni modeste (V, 255 LI 8-13). Mais si l'on cherche les raisons profondes de son attitude, on est ramené à ceci : alors qu'un savant comme GALILÉE rendait compte des phénomènes, c'est-à-dire les calculait, DESCARTES prétendait les expliquer et croyait pouvoir en découvrir clairement les causes, qu'il fut trop souvent enclin à deviner. Refusant de se contenter des faits, il eut recours à des hypothèses explicatives revêtant toujours la forme d'un modèle, c'est-à-dire d'une image idéale soumise à des lois qui sont identiquement celles que suivent les données de l'expérience; cependant, ainsi que nous l'avons dit dans le paragraphe relatif à DESCARTES et KÉPLER, cette méthode spéculative à laquelle DESCARTES s'est principalement tenu sans la faire alterner avec une philosophie plus positive constitue un aspect du procédé par analogie. DESCARTES usa de métaphores pour décrire le domaine idéal où il se complaisait; voilà sans doute pourquoi il n'a pas plus relevé les lois de KÉPLER sur le mouvement des planètes (87) que la loi de GALILÉE sur la proportionnalité de la longueur du pendule au carré de la période des oscillations : « Ie ne dis rien des demonstrations de Geometrie dont la plus part de son livre est rempli (les *Discorsi* de GALILÉE), car ie n'ay

(84) Rappelons que c'est par prudence aussi que ROBERVAL exposait, sous le masque peu compromettant d'Aristarque, ses idées sur le système du monde : *Aristarchi Samii De mundi Systemate...* 1644.

(85) « La question de religion ne fut pour rien dans ce qui l'attira en un pays ou le détourna d'un autre » (XII, 69).

(86) Cf. E. GILSON, Descartes en Hollande; *Revue de métaphysique et de morale*, t. 28, 545-556, 1921.

(87) La recherche des causes a été indifférente à KÉPLER, que domina l'idéal esthético-mathématique des Grecs.

sceu auoir la patience de les lire, & ie veux croire qu'elles sont toutes vrayes » (88).

Le savant doit être humble vis-à-vis de la nature. Il ne peut se permettre de donner libre cours aux suggestions les plus séduisantes de l'intellectualité. Un doctrinarisme intransigeant, auquel les savants eux-mêmes n'échappent pas, aveugla BACON et DESCARTES avec une force qui nous paraîtrait singulière aujourd'hui si nous ne nous rappelions qu'au dix-septième siècle l'obscurité régnait encore de tous côtés au point que — faute des quelques hypothèses et principes directeurs qui nous rendent à présent la science plus facile, sinon la réalité plus simple — la complexité des phénomènes n'était pas soupçonnée. Il y aurait mauvaise grâce de vouloir reprocher aux pionniers de n'avoir pas connu cette probité que DARWIN rendit si émouvante lorsqu'il s'attachait scrupuleusement à mettre en relief la différence qu'il y a entre la spéculation et un fait : « When a difficulty or an objection occurred to him, he thought it of paramount importance to make a note of it instantly, because he found hostile facts to be especially evanescent » (89).

La science expérimentale est une école de modestie. Ajoutons que c'est probablement la raison pour laquelle elle est née dans une atmosphère où telles vertus ignorées des Grecs : le renoncement, une douce patience, étaient dominantes. Le christianisme, à ce point de vue, lui a été favorable, et, selon toute apparence, elle n'en a retiré que ce seul bénéfice.

Peut-être aura-t-on vu non sans surprise BACON unanimement délaissé par ses contemporains. C'est qu'en réalité, comme l'a très justement noté LIEBIG, il « ignorait complètement la véritable tâche de la science, ainsi que les moyens de réaliser cette tâche » (90). Alors que l'influence de DESCARTES sur la physique et l'astronomie est intimement liée au progrès de ces sciences, l'absence totale de vertu féconde et créatrice dans l'œuvre de BACON est révélée dès le début par le fait qu'il n'a pas su créer d'école. Au surplus, quand on considère la très grande variété de tendances et de directions qui se manifestent à une époque déterminée dans les expres-

(88) *Œuvres* de DESCARTES, édit. ADAM-TANNERY; II, 388.

(89) *The Life and Letters of* CH. DARWIN, by FR. DARWIN; sec. edit., vol. III, London, 1887, p. 54. Cette référence m'a été indiquée par MR. SARTON.

(90) LIEBIG, *loc. cit.*, p. 127.

sions de la pensée ayant la science comme but final, il est aisé d'admettre que certaines d'entre elles aient pu passer inaperçues. Or, ces courants intellectuels furent tels que les découvertes ont germé et se sont développées dans une indépendance absolue. Nous croyons qu'il est temps de recourir à une méthode nouvelle dans l'étude de la constitution de la science au dix-septième siècle. Il conviendrait de faire prédominer l'étude des sentiments et des faits *communs* aux principaux représentants de la science à cette époque. Ce n'est pas parce que le grand siècle nous apparaît comme l'âge des fortes personnalités, des individualités puissantes, qu'il est profitable et logique de les étudier séparément, en augmentant ainsi les chances de les isoler, de n'en signaler que des aspects limités et peut-être secondaires. Tant d'idées communes et cependant, dans l'exemple particulier que nous avons choisi, la plus grande indifférence se traduisant par l'absence de toute relation entre savants, n'est-ce point là la preuve de l'unité des tendances profondes de la pensée scientifique et l'indice d'une inspiration commune ? Et cet étonnant mystère subsiste : la conscience collective d'une œuvre à accomplir. Dans toute création collective, il y a un miracle. Mais les miracles sont nombreux dans l'histoire du triomphe de la raison.

Harvard University. J. PELSENEER
(Fellow of the C.R.B. Educational Foundation)

Section of the History of Science of the Carnegie Institution

Thirteenth annual report for the period extending from July 1, 1930 to June 30, 1931 (previous reports appeared in Year Books nos. 18-29; the twelfth also in *Isis*, 15, 170-71).

The Division of Historical Research of the Carnegie Institution organized in 1903, reorganized in December 1929, is now divided into three sections : 1. Section of aboriginal American history; 2. Section of United States history; 3. History of Science. Or otherwise : American prehistory; American history; History of civilization.

The first section is by far the most important and has the largest budget as excavations and field work in distant parts are necessarily very expensive. Its leader, Dr. ALFRED VINCENT KIDDER, is the chairman of the whole Division.

Our section includes the following members : GEORGE SARTON; ALEXANDER POGO; MARY CATHERINE WELBORN, secretary.

Two associated investigators, W. A. HEIDEL and ELIAS A. HOWE have dedicated themselves to the history respectively of Greek thought, and of Latin palaeography anterior to the ninth century.

For the reports of other sections and of the associated investigators see the Year Books of the Carnegie Institution.

To return to our own section : Dr. POGO was absent, travelling in Europe, from the end of July to the end of October 1930; he visited various libraries in Italy and Germany. Miss WELBORN began her work on October 1, 1930. Dr. JOHN PELSENEER of Brussels and Miss C. DORIS HELLMAN of New York worked throughout the year as voluntary apprentices.

Dr. SARTON plans to sail from Quebec on July 17, 1931, and after having taken a long deferred and much needed vacation in Europe, to proceed to the Near East and arrive in Beirut about

October 1. His headquarters in the Near East throughout the academic year 1931-32 will be the American University of Beirut, whose generous coöperation is much appreciated.

The purpose of this relatively long stay in Syria is to obtain a better knowledge of Arabic and incidentally of Hebrew, and some familiarity with the spiritual conditions obtaining in the Near East to-day. This is becoming urgent because these conditions are now changing at a terrific pace, the main causes of change being the cheap motor cars and the movies. SARTON has proved, in vol. 1 of his *Introduction to the History of Science*, that for a period of four centuries (from the eighth century to the eleventh), Arabic was the main language of science and the main vehicle of progress. Even as to-day any oriental wishing to obtain the best information on any subject and to remain abreast of the times must begin by mastering one of the main western languages, even so in those centuries the shortest road to knowledge was the study of Arabic. In the twelfth century that situation was gradually altered, and by the end of it, Jews, Christians and Muslims were already competing for intellectual supremacy. In order to qualify for that competition Jews and Christians were obliged first of all to translate into Hebrew and into Latin the treasures of knowledge preserved in the Arabic writings. It is typical that it was only by slow degrees that they realized the need of translations made directly from the Greek. So great was the prestige of Arabic writings in the twelfth and thirteenth centuries that western scholars spent most of their energy in translating these writings, though the majority were themselves translations from Greek originals which were still available, or might have become available if there had been a sufficient need of them. Since that time many Greek originals have been lost, for example, many were wantonly destroyed when the Crusaders sacked Constantinople in 1204. Historians have generally considered the fall of Constantinople under Turkish onslaught in 1453 as one of the greatest landmarks in the world history; indeed that date has often been taken as the conventional upper limit of the Middle Ages and the beginning of the Renaissance. In fact the first fall of the city in 1204 was even more important : this was like a final blow to the old Greek culture; it completed the severance between the Greek and Latin worlds.

Happily by that time a great many Greek and Arabic works had become available to the West through Latin and Hebrew translations made most of them from the Arabic. Volume 2 of SARTON's *Introduction* is largely devoted to the comparative study of the Latin and Hebrew traditions of Greco-Arabic science. From the thirteenth century on the Arabic world lost its spiritual hegemony but remained very important, and some of the leading scientific treatises of the thirteenth and fourteenth century were still written in Arabic.

It is for these reasons that the evolution of mediaeval thought can hardly be understood without reference to the Arabic sources, and that the study of Arabic language and of the Muslim civilization is of such fundamental importance. During his stay in the East Dr. SARTON will try to read, speak, and write as much Arabic as possible under the guidance of Syrian instructors. During the past five years he has studied Arabic almost entirely alone, most of his time and energy being taken by other phases of his work; during the academic year 1931-32, that proportion will be reversed, and most of his time will be spent in Arabic research. Even so he is not too sanguine of success. The Arabic language is as intricate as it is beautiful; the Arabic literature is immense; a life time is hardly long enough to attain mastery in these fields even if one's attention is not constantly diverted into other directions. It should be noted that practically every Arabic scholar in western countries is almost exclusively an Arabist or Orientalist, while Dr. SARTON is primarily a historian of science, and Arabic science, however important, is but a part, a small one at that, of the whole past of science. Yet this Arabic year will make a good deal of difference. Dr. SARTON will be satisfied if it enables him to read Arabic documents with greater speed and confidence. Moreover the best fruits of such an undertaking are not so much definite items of information as more or less unconscious inductions, impressions and intuitions. While he realizes that an academic year (that is, less than nine months) is but a very short time for such an ambitious program as his, yet he hopes that the additional knowledge and understanding thus obtained will make it possible for him to speak of eastern science and eastern thought with greater assurance and authority. Accepting humbly the narrow

limitations of his own efforts, he also hopes that this year of eastern life will enable him to know more exactly what should be done in the near future by himself and by others.

It cannot be emphasized too strongly that what is at stake is not simply a matter of knowledge, but a matter of understanding and good will. The conflict between East and West is but a phase of the eternal conflict within our own western world between literary and artistic pursuits on the one hand and scientific and technical ones on the other, or to put it otherwise between the old fashioned humanism and the new. A protracted war affects mankind almost in the same way as a mortal disease affects a single man if he does not die of it. The disease itself is followed by a period during which the main danger is averted but the normal equilibrium is not yet attained. This our time is such a period of convalescence. The world is out of joint materially, intellectually, spiritually. Our investigations are of no use as far as the material well-being of mankind is concerned, but they may be of value with regard to its spiritual conditions. There is a widening gap between the literary and scientific élites, between the Easterners and the Westerners, which it is urgent to bridge. The best way of bridging it is to inject a little of the humanistic spirit in our scientific studies, that is, to study not only the latest results of science (as scientists do) but the whole of its evolution. The study of mediaeval and of oriental science is particularly useful as Dr. SARTON has explained it in his Colver Lectures in Providence last year.

The work done by members of this section during the year 1930-31 will be summarized under four headings as usual.

1. *Introduction to the History and Philosophy of Science.*— Dr. SARTON completed the MS. of the second volume dealing with the twelfth and thirteenth centuries, and prepared its final redaction. It was accepted for publication by the Institution and much time was spent in reading the proofs. In this the author was very ably helped by Miss MARY C. WELBORN who read all of the proofs and by Dr. A. POGO, who read some of them. He is indebted to both for many valuable suggestions. The index is now being compiled under the author's direction by Miss FRANCES SIEGEL (Radcliffe '31). This volume will appear during the summer 1931 (in two parts, 1250 pages).

Miss WELBORN has begun a revision of the notes relative to the Latin astronomical writings of the fourteenth century, for the third volume.

Dr. POGO has continued investigations on science in the first half of the sixteenth century, for the fifth volume. Incidentally, he began a study of an unpublished MS. of ORONCE FINÉ : *L'art et manière de trouver la longitude* (1543).

2. *Other investigations.*—Professor J. PLAYFAIR MCMURRICH's study of the anatomical MSS. of LEONARDO DA VINCI undertaken in 1919 was finally completed and published under the Institution's auspices.

Dr. SARTON continued his examination of some of the fundamental memoirs of modern science, to wit, those of NEWTON on the dispersion of light and the nature of color (1672), of JAMES BRADLEY on the aberration of light (1729) and the nutation of the earth's axis (1748), of ALESSANDRO VOLTA on the electric cell (1800), of KARL ERNST V. BAER on the mammalian egg (1827), of WILLIAM ROWAN HAMILTON and HUMPHREY LLOYD on conical refraction (1833). The purpose of these examinations is twofold. First, to draw attention, to the constant need of returning to the original sources, the main infirmity of historical studies in general and of our studies in particular being their secondhandedness. Of course some amount of secondhandedness is unavoidable on account of the brevity of life, but the duty of avoiding it as often as possible is nevertheless imperative. Second, to oblige himself to devote at least a part of his time to the study of modern science and thus to avoid the lopsidedness which might be caused by exclusive devotion to mediaeval thought. While he is naturally obliged to concentrate his efforts on a certain period the historian of science must try to keep his interest alive to the events of every period of the past and the present; it is only by so doing that he will be able to understand the implications of human progress for each period helps to explain every other.

Dr. POGO pursued his investigations relative to the development of Egyptian astronomy during the IXth to the XIth and during the XVIIIth and XIXth dynasties. He had occasion to review the very abstruse researches of the late CARL SCHOCH on the application of astronomical methods to the determination of ancient chronology.

Miss WELBORN has finished a book begun in Paris under the direction of the late Professor LOUIS J. PAETOW, on Calendar reform in the thirteenth century, including an edition and translation of the *Compotus* of ROBERT GROSSETESTE, which is to be published in the Records of Civilization Series, Columbia University Press. She has also completed an investigation which she had begun at the University of Chicago under the direction of Professor JAMES WESTFALL THOMPSON, on the development of Arabic influences in Lotharingia in the eleventh century; it appeared in *Isis*, 16, 188-199. She has completed an English translation with notes and glossary of ROGER BACON's treatise *De erroribus medicorum* to appear in *Isis*.

Dr. JOHN PELSENEER, Fellow of the C.R.B. Educational Foundation, studied HOOKE's unpublished diaries, various autographs of NEWTON, some details in the development of mathematical notations, and analyzed the interdependence and mutual judgments of the leading scientisits in the first half of the seventeenth century. He examined the possibility of preparing a complete edition of NEWTON's correspondence, and made the first efforts to change that possibility into a reality.

Miss C. DORIS HELLMAN (Vassar '30) completed her study of the life and work of the English instrument maker, GEORGE GRAHAM (1673-1751) and made a similar investigation with regard to a follower of GRAHAM, JOHN BIRD (1709-76). These investigations are of value for the understanding of the development of astronomy in the eighteenth century. She began a study of the history of comets and analyzed the contents of some fifteen contemporary tracts dealing with the comet of 1577. She tried to determine the nature of THOMAS JEFFERSON's efforts toward the standardization and decimalization of American weights and measures. She edited an unpublished diary of EDWARD JENNER. Finally she made various minor investigations for herself and for Dr. SARTON.

Mr. DANA B. DURAND prepared a preliminary bibliography for the history of geography from c. 1300 to 1492 on the basis of the notes accumulated for vols. 3 and 4 of SARTON's *Introduction*.

3. *Publication of Isis.*—During the past year three numbers of *Isis* have been published (44 to 46) forming the end of vol. 14 (pages 277-582) and the main part of vol. 15 (504 pages), a total

of 810 pages, 25 plates, 5 figures. They contain 19 papers, 59 shorter communications and reviews, and 2143 bibliographic notes. A large part of the editorial burden was kindly assumed by Dr. POGO.

Thus far the Editor had been assisted by Mr. L. GUINET, managing editor in Brussels, and by three associate editors (the first three in the following list). In view of the editor's absence in the East during the year 1931-32, and in order to establish the journal on a sounder foundation, the board of associate editors has been enlarged. It now includes eleven members as follows :

C. H. HASKINS (Middle Ages)
R. C. ARCHIBALD (Mathematics)
J. K. WRIGHT (Geography)
D. B. MACDONALD (Islam)
C. A. KOFOID (Biology)
LYNN THORNDIKE (Middle Ages)
SOLOMON GANDZ (Israel)
TENNEY L. DAVIS (Chemistry)
H. T. DAVIS (Philosophy of science)
A. POGO (Astronomy)
C. D. LEAKE (Medicine).

The Carnegie Institution has no direct interest in *Isis*, but cannot help being indirectly concerned for its welfare, because the publication of *Isis* and of SARTON's *Introduction* are in the deepest sense two complementary undertakings. The *Introduction* could not be half as elaborate and complete if its author was not also the editor of *Isis* and in that capacity the recipient of valuable and abundant information streaming to him continually from every part of the world. On the other hand, the errors which are unavoidable in a work of such scope are corrected in *Isis* as soon as possible. Hence any owner of the *Introduction* can keep his own copy of it up-to-date or obtain the latest information on any single topic by examining the numbers of *Isis* as they appear.

4. *Lectures.*—Dr. SARTON delivered a course of some thirty-five lectures on the history of science in the eighteenth and nineteenth centuries at Harvard University, and another course of equal length on the history of mathematics at Radcliffe College. Dr. POGO gave an illustrated lecture on Egyptian astronomy at the Harvard College Observatory, and Dr. PELSENEER discussed informally Hellenic science before the Harvard Classical Club.

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La introducción del cuadrante con cursor en Europa

SUMARIO

Estado actual del planteamiento del problema. La *Geometria incerti auctoris* y la cuestión de la determinación de la hora por medio del cuadrante con cursor. Su relación con otras cuestiones geométricas eliminadas de los textos sucesivamente editados de la *Geometria incerti auctoris*. Versiones *A* y *B* del pasaje de la determinación de la hora por medio del cuadrante con cursor. Existencia, en el siglo X, de versiones latinas de tratados sobre el cuadrante con cursor. Versión *C* sobre la estructura de un cuadrante con cursor. Versión atribuida a HERMANN CONTRACTO. Identificación de un tipo de cuadrante con cursor *uetustissimus*, análogo al *uetus*. Comparación de los dos tipos. Aplicaciones astronómicas del cuadrante *uetustissimus*. Filiación de las versiones latinas del cuadrante *uetustissimus*: su relación con las traducciones del árabe hechas en Cataluña en el siglo X. Incierta fuente de nuestro tratado sobre el cuadrante *uetustissimus*. Relaciones del cuadrante *uetustissimus* con otros cuadrantes de origen árabe. Desplazamiento del cuadrante *uetustissimus* por el *uetus*; igual sustitución en el cuadrante del dorso de la azafea de AZARQUIEL. Dificultades que ofrecen las alusiones al cuadrante en la obra sobre astrolabio de MACHALA.

El problema de la introducción del cuadrante con cursor en la Europa medieval parece haber sido soslayado por unos autores, mientras que por otros se ha declarado insoluble, dados los escasos datos hasta el presente conocidos. Esta es la opinión que nos hemos formado después de la lectura de las obras generales sobre Historia de las ciencias matemáticas y físicas (1) y aún de monografías dedicadas al estudio de cuadrantes célebres, que nos han

(1) D. E. SMITH, *History of Mathematics*, Boston, 1923-1925. Vid. la sección: *Instruments in Geometry* del cap. V del vol. II, pág. 344 sigs. M. CANTOR en su monumental *Vorlesungen über Geschichte der Mathematik*, 3ª ed. Leipzig 1907-1908, parece aludir sólo a las aplicaciones geométricas del cuadrante, y por tanto no se fija en al cuadrante con cursor, de índole más especialmente astronómica. M. DELAMBRE, *Histoire de l'Astronomie du Moyen Age*, Paris, 1819, pág. 247 del vol. II, deja en interrogante el origen del cuadrante con cursor.

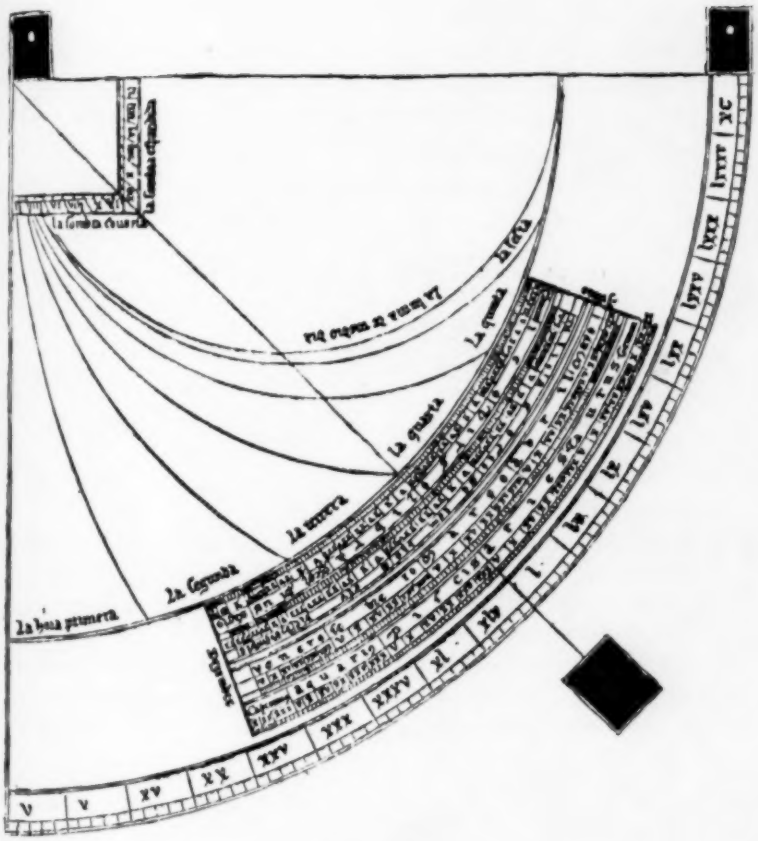

FIGURA I

Isis, XVII, pl. 11. J. MILLÀS VALLICROSA.

Figura de cuadrante con cursor, que aparece en el *Libro del cuadrante*, redactado por RABÍ ÇAG a instancias de ALFONSO EL SABIO (Cf. la edición de M. RICO SINOBAS : *Libros del Saber de Astronomía*, vol. III, p. 306.)

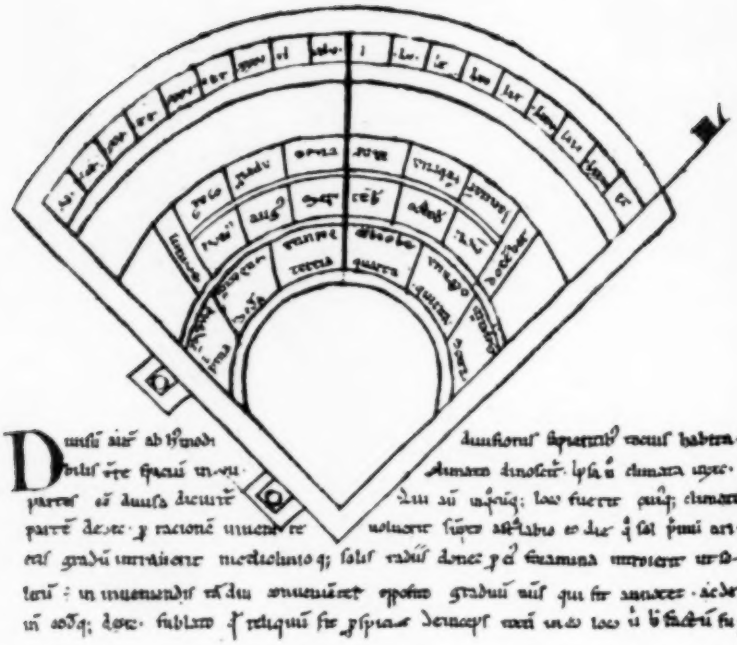

FIGURA II

Isis, XVII, pl. 12. J. MILLÀS VALLICROSA.

Figura de cuadrante con cursor, que aparece en el Ms. de Londres, Brit. Mus. Old Royal, 15 B IX (siglo XI), fol. 71*r*.

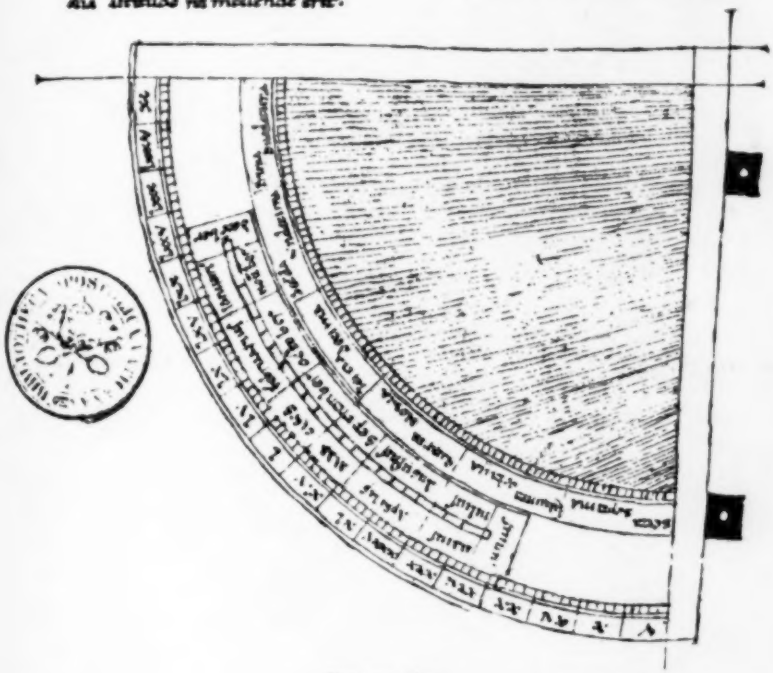

FIGURA III

Isis, XVII, pl. 13. J. MILLÀS VALLICROSA.

Figura de cuadrante con cursor, que aparece en el Ms. de Roma, Vatic. Regin. 1661 (siglo XI ex.), fol. 86*v*.

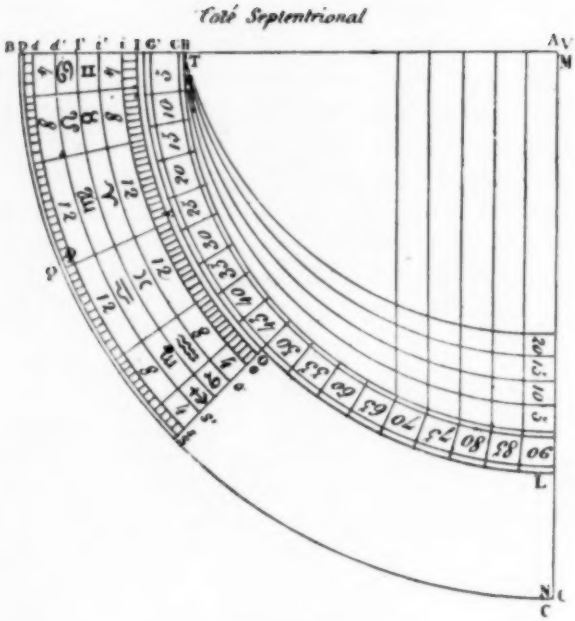

FIGURA IV

Isis, XVII, pl. 14. J. MILLÀS VALLICROSA.

Cuadrante de AZARQUIEL (apud SÉDILLOT, *op. cit.*, fig. 11.)

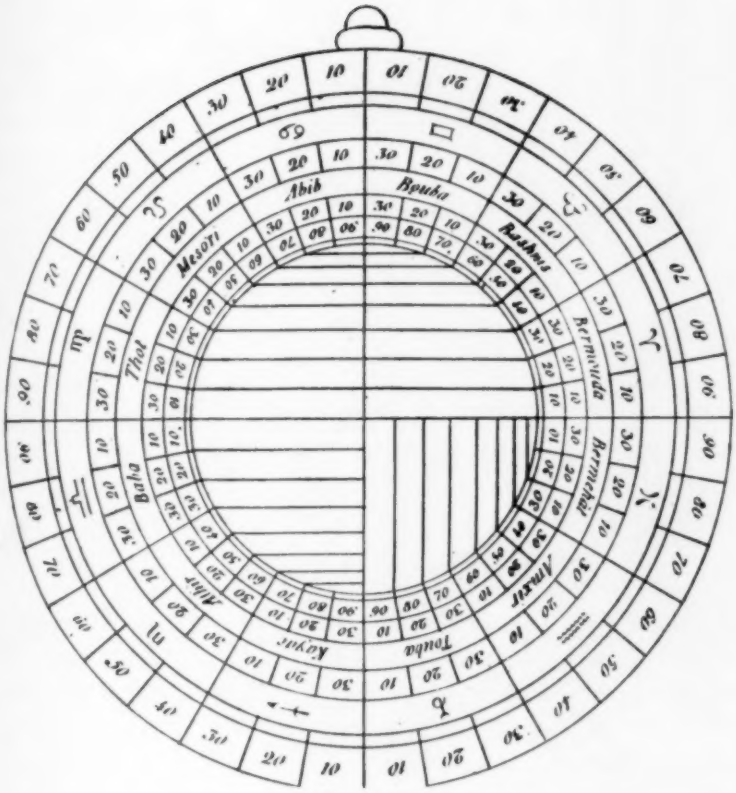

FIGURA V

Isis, XVII, pl. 15. J. MILLÀS VALLICROSA.

Cuadrante con las lineas de proyección, del dorso de la azafea de AZARQUIEL (cf. SÉDILLOT, *op. cit.*, fig. 94.)

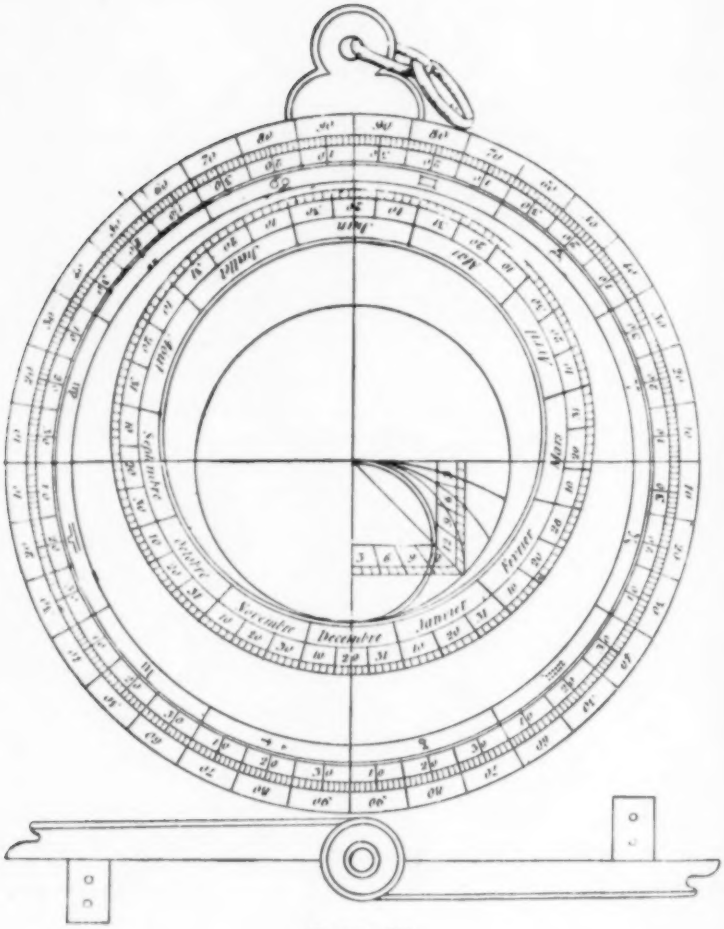

FIGURA VI

Isis, XVII, pl. 16. J. MILLÀS VALLICROSA.

Cuadrante con las lineas horarias, del dorso de la azafea de AZARQUIEL (cf. SÉDILLOT, *op. cit.*, fig. 96.)

llegado como testimonios de la ciencia y técnica medievales (2). Habiendo encontrado en el transcurso de nuestras largas búsquedas sobre historia de las ciencias en Cataluña, nuevos datos sobre aquella cuestión, los cuales proyectan una nueva luz, nos hemos permitido destacarlos y proponer la siguiente visión del problema con vistas a su solución.

Dede luego que por cuadrante con cursor o cuadrante de corredera entendemos el cuadrante que junto a las lineas horarias, plomada y pínulas de mira, reune la pieza móvil : cursor o corredera, en la cual están relacionados los días de los doce meses del año con la posición del sol en cada uno de los signos del zodíaco, de modo que deslizándose el cursor a lo largo del limbo graduado del cuadrante, permitía sumar o restar autómaticamente la declinación del sol respecto de la altura observada. La importancia práctica de este cuadrante es que el observador no tenía que recurrir a unas tablas de declinación para completar les datos obtenidos por la operación, sino que el mismo aparato le daba el cálculo ya hecho. La finalidad, pues, del cuadrante con cursor no era simplemente topográfica o geométrica, sinó preferentemente astronómica : determinación de la hora «temporal», determinación del clima o latitud de una ciudad o región, a base de la elevación del sol y viceversa, etc. Nuestro ALFONSO EL SABIO en su gran obra *Libros del saber de Astronomía*, dedica un libro a nuestro cuadrante : «*Libro del cuadrante pora rectificar*», dividido en dos partes—según la división entonces clásica en todas las obras de astronomía — : una de ellas relativa al modo de construcción del instrumento, y la otra relativa a sus aplicaciones; y dice el Rey en su prólogo : «et porque esta parte primera deste libro non fué fallada en esta sazon de agora cierta et complida assi cuemo deue seer, por ende nos Rey Don ALFONSO el sobredicho mandamos a nuestro sabio RABI ÇAG el de Toledo que lo fiziese bien cierto et bien complido». (3)

(2) P. TANNERY, *Le traité du Quadrant de Maitre Robert Anglés (Montpellier, XIII siècle). Texte latin et ancienne traduction grecque*. París, 1897 (Separata de *Notices et Extraits des Manuscrits de la Bibliothèque Nationale et autres bibliothèques*, tomo XXXV, 2ª parte). A. ANTHIAUME y J. SOTTAS, *L'astrolabe-quadrant du Musée des Antiquités de Rouen*. París, 1910.

(3) M. RICO SINOBAS, *Libros del saber de Astronomía del Rey D. Alfonso X de Castilla*, Madrid, 1863. Véase el reciente estudio de J. SORIANO VIGUERA, *La Astronomía de Alfonso X el Sabio*. Madrid, 1926, págs, 152 sigs.

Este cuadrante de que hablamos, o sea, el mismo descrito en la obra del Rey Sabio, fué conocido por los autores de ciencia medievales con el nombre de *quadrans uetus* o *quadrans antiquus*, en oposición al *quadrans nouus* que desde el principio del siglo XIV se destaca de aquél. El *quadrans nouus* fué debido a una feliz innovación del célebre judío Don PROFIAT TIBBÓN (4), quien en Montpeller — dependiente entonces de la Corona de Aragón — y hacia el año 1290, estableció las reglas para asociar al cuadrante algunos elementos del astrolabio planisférico, y dotar al primero, de ciertos elementos de índole trigonométrica, para obtener los senos y senos versos de un arco dado. De esta manera el cuadrante vino a merecer la preferencia de los astrónomos con respecto al astrolabio, puesto que en igualdad de dimensiones el cuadrante permitía obtener una aproximación mucho mayor; y no es de extrañar que el nuevo cuadrante se encontrara por doquier entre las manos de navegantes y astrónomos hasta bien entrada la Edad Moderna.

Tal es la trayectoria de cuadrante, con cursor o ya sin él, del cual, según hemos dicho, se desconocía su introducción en Europa. Vamos a ver si podemos seguir sus primeros pasos.

El primer texto medieval en el cual se hace referencia expresa al cuadrante con cursor es un texto relacionado con la denominada *Geometria incerti auctoris*, o sea, con la pseudo-geometría de GERBERTO. Es cosa bien conocida las polémicas habidas entre los historiadores de las matemáticas acerca de la autenticidad o inautenticidad de este tratado de Geometría respecto de GERBERTO; en un principio, y sobre todo por la escuela de CANTOR, no se había dudado de la atribución de esta obra a GERBERTO, y se pretendía no ver en ella sinó un fruto de la tradición científica latina, una derivación de la técnica de los antiguos gramáticos. Todo el peso enorme de la autoridad de CANTOR estaba en favor de esta solución. Pero un estudio profundo de su estructura interna, de sus métodos, de sus deficiencias, de sus fuentes indujo a ver en ella faltas de continuidad, disociaciones de doctrina, todo lo cual no se compadecía con la afirmación de la unidad de

(4) Sobre la producción científica de este autor cf. M. STEINSCHNEIDER, *Die hebraeischen Uebersetzungen des Mittelalters und die Juden als Dolmetscher*. Berlin, 1893. Págs. 607, 976.

la obra, con su atribución a GERBERTO, ni con su única y exclusiva derivación de fuentes latinas. La crítica paleográfica vino a confirmar después esta segunda visión del problema, y la ingente labor de cotejo y crítica de mss. realizada por BUBNOV — el editor de las obras matemáticas de GERBERTO (5) — estableció de un modo definitivo que la parte comprendida entre los capítulos XVI y XCIV de la Geometría atribuida a GERBERTO, no pertenecía a la obra de éste, sinó que era parte de una obra anónima, la cual probablemente había servido de fuente a la parte realmente legítima de la obra de GERBERTO. A esta parte ilegítima dió BUBNOV el nombre de *Geometria incerti autoris* y con este nombre se la conoce.

Ahora bien, la primera parte de la *Geometria incerti auctoris* (correspondiente a los antiguos caps. XVI-XL) en rigor no viene a ser más que una colección de problemas de topografía o agrimensura. En ella se trata de metir distancias, alturas accesibles o inaccesibles, profundidades, etc., valiéndose de una serie de procedimientos é intrumentos característicos : Medición de una altura por medio de la sombra proyectada; medición de una altura, accesible o inaccesible, por medio de un espejo o bien de un recipiente lleno de agua; medición de una distancia o de una altura, accesibles o inaccesibles, por medio de una o dos estacas; medición de una altura por medio de un triangulo rectángulo; medición de distancias por medio de un cuadrilátero rectángulo; y medición de distancias y alturas, accesibles o inaccesibles, por medio del astrolabio.

La derivación arábiga de la mayor parte de estos procedimientos de medición está del todo evidenciada después de los trabajos de WEISSENBORN (6), corroborados en nuestros dias por WÜRSCHMIDT (7). Y se ha comprobado que la presencia, en algunos manuscritos, de la palabra arábiga *alhidada* no se debía a alguna nota o adición de los copistas posteriores, sinó que debería ser originaria.

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(5) N. BUBNOV, *Gerberti opera mathematica*. Berlín. 1899. Págs. 46 sigs.

(6) H. WEISSENBORN, *Gerbert. Beiträge zur Kenntnis der Mathematik des Mittelalters*. Berlín, 1888. Vid. especialmente el cap. II; págs. 97 sigs.

(7) J. WÜRSCHMIDT, *Geodätische Messinstrumente und Messmethoden bei Gerbert und bei den Arabern* (*Archiv der Mathematik und Physik*, vol. XIX, 1912, págs. 315-320).

Pues bien, en esta parte de la *Geom. inc. auct.*, entre los capítulos VI y VII (Caps. XXI y XXII de la ed. de Pez y caps. XXII y XXIII de la ed. de OLLERIS (8)) aparece un capítulo sobre el modo de encontrar la hora por medio del cuadrante con cursor. No en todos los Mss. de la *Geometria incerti auctoris* aparece dicho capítulo, pero sí en buena parte de ellos, los que han sido clasificados por BUBNOV (9) con la notación *DB*. Sin embargo, el mismo capítulo aparece en otros Mss. — de carácter misceláneo, como la mayor parte de los de esta clase — si bien no formando parte del texto de la *Geom. inc. auct.*, sí puesto casi a continuación de la misma, alternando con textos de índole geométrica o astronómica. Tal ocurre en el Ms. de Londres, British Museum, Old Royal 15 B IX (siglo XI), en el cual el texto de la *Geom. inc. auct.* ocupa los folios 61r.-68v., siguen luego algunos aditamentos a dicha Geometría, dos cartas cruzadas entre ADELBOLDO DE UTRECHT y GERBERTO, sobre temas de Geometría (10), y acto seguido, en el folio 70r., aparece nuestro texto sobre determinación de la hora por medio del cuadrante con cursor. Claro está que no siendo la finalidad de este capítulo de índole geométrica, sinó astronómica, y apareciendo en el cuerpo de la *Geom. inc. auct.* tan sólo en una familia de manuscritos, los sucesivos editores de ésta, BUBNOV inclusive, se creyeran en el caso de separar nuestro capítulo y considerarlo más bien como una interpolación.

Pero es el caso que algunos otros capítulos, de índole ya estrictamente geométrica, o mejor geodésica, fueron eliminados del cuerpo de la *Geom. inc. auct.*, por los últimos editores de ésta : OLLERIS y BUBNOV, fundándose probablemente en que no aparecían aquellos capítulos en la totalidad de los Mss.

Uno de estos caps. trata del empleo de un cuadrado dotado de varillas o pínulas de mira, para la medición de distancias; se encuentra no sólo en la familia de Mss. *DB*, sinó en bastantes otros de los siglos XI-XII, anotados por BUBNOV con la signatura *EA* y *EB* (11); pero tambien lo encontramos en un Ms. de Ripoll, el nº 225, casi desconocido por los historiadores, y el más antiguo

(8) A. OLLERIS, *Œuvres de Gerbert*. París, 1867. Pág. 433.
(9) *Op. cit.*, pág. 313.
(10) Publicadas por BUBNOV, *op. cit.*, págs. 41-45 y 300-309.
(11) *Op. cit.*, pág. 315.

de toda la serie de estos Mss, pues es del siglo X, en el cual (fol. 20r.-20v.) aparece aquel capítulo junto con otros de la *Geom. inc. auct.* y con pasajes de tratados de astrolabio, desde luego de tradición árabe. La técnica de este cuadrado es muy semejante a la de los otros cuadrados empleados en la misma *Geom. inc. auct.* (12).

El otro capítulo, que igualmente fue desglosado del cuerpo de la *Geom. inc. auct.*, trata del modo de medir grandes profundidades v. gr. de mares, lagos, etc., valiéndose de un procedimiento muy ingenioso : Se soltaba en la superficie del mar o lago cuya profundidad se quería medir una bola de cobre, hueca, dispuesta en un armazón conveniente de modo que al bajar uniformemente hacia el fondo y tocar a éste, inmediatamente y de un modo automático se soltaba y volvía a la superficie. Si sabíamos la hora astronómica exacta del momento en que fué sumergida la bola al agua y asimismo sabemos la hora exacta de su reaparición a la superficie del agua, tendremos que la relación que haya entre el espacio de tiempo transcurrido entre aquellos dos momentos y otro espacio de tiempo que se tome por unidad — equivalente a una profundidad dada — nos dará la profundidad que buscamos. También dice el texto de dicho capítulo, que se puede medir el tiempo entre aquellos dos momentos, valiéndose de aparatos a manera de clepsidras (13).

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(12) En nuestro estudio aludido sobre historia de las ciencias matemáticas en Cataluña nos fijamos detenidamente en este cuadrado. Vid. la versión que da, en apéndice, BUBNOV, *op. cit.*, pág. 365.

(13) Por haber dado OLLERIS, *op. cit.*, pág. 446, nota 2, un texto no definitivo, nos permitimos dar aqui la versión segun el ms. 225 de Ripoll, fol. 21*v*., concordante con bastantes otros mss. :

DE PROBANDA PROFUNDITATE

Quando uis scire profunditatem alicuius pelagi uel stagnu siue fluminis construe tibi globum de eramine, aut de plumbo in modum subtus scriptum undique rotundum et tenuem quantum potes fieri. Hoc facto, construe etiam et aliam formulam ex ferro secundum quod infra scriptum est taliter ut A B latior fit quam C D et inter A C et B D maioris longitudinis fiat quam inter A et B. Et ex parte A habeat ungulam per que dimittitur globus in profundum cuiuslibet rei et ex parte C clauum protensum usque ad E cuius caput grossius sit qua cetera et qualitas propter ciciorem immersionem. Quo facto cum altum cuiuslibet fluuii scire uolueris, inmitte globum cum ferro in aqua et in ora immersionis perspice in astrolabio que ora sit. Postquam uero offensione fundi sua sponte regreditur, iterum considera, quot hore transacte sint in astrolabio inter immersionem et emersionem. Postea accipies astam siue aliam mensuram, et inmerge in eo loco probando quot pedes uel cubitos habeat illa profunditas, ut scias quot pedes

Este segundo capítulo, de caracter geodésico, aparece en bastantes Mss. integrando el texto de la *Geom. inc. auct.* (14), y en otros Mss. en inmediata relacion con ella. En el referido ms. nº 225 de Ripoll tambien aparece (fol. 21v.-22v.) junto a otros caps. de la *Geom. inc. auct.*. De manera que podemos dar por bien fundamentado que estos dos caps. corresponderían al cuerpo de la *Geometria incerti auctoris*, y que no había razon suficiente para desglosarlos del mismo tal como hicieron los citados editores.

Ahora bien, bastantes de estos Mss. que presentan este capítulo de medición de profundidades según el espacio de tiempo transcurrido entre las dos horas de inmersión y emersión, ofrecen inmediatamente junto a él, aquel pasaje sobre la determinación de la hora, por medio del cuadrante con cursor. De modo que creemos que este pasaje del cuadrante con cursor — de finalidad astronómica — estaría realmente relacionado con aquel capítulo de medición de profundidades, por lo menos en la fuente — seguramente oriental — de la que emanan los caps. de la primera parte de la *Geom. inc. auct.* Los sucesivos copistas irían eliminando luego este pasaje astronómico del cuadrante, por rebasar, segun ellos, el fin estrictamente mensurativo ó topografico de aquella parte de la *Geom. inc. auct.* Pero repetimos que los Mss. que a nuestro parecer guardan una mayor fidelidad a la fuente primitiva — casi todos ellos del siglo XI — ofrecen el pasaje del cuadrante con cursor inmediatamente o casi inmediatamente con el capítulo de medición de grandes profundidades.

Pero este pasaje relativo al cuadrante con cursor se presenta

uel cubiti dandi sunt unicuique hore cum uentum fuerit ad maioris fluuii profundum. Nam in paruo debent prius predicta fieri. Quod si hoc non queris facere, accipe uas tellureum subtus perforatum, et pone super aquam in inmersione globi cum ferro, et uidebis quantam colligat aquam quousque redeat ferrum et ponderando ipsam aquam dabis ad IIII^{or} argenteos x^{em} status hominis medii, id est ad unum argenteum II et dimidium ad CC argenteos D et sic probabis.

El resto que acompaña en la versión de OLLERIS no corresponde en modo alguno a este pasaje. El ms. 11248 F.L. de la Bib. Nat. de Paris, fol. 27*r*-27*v*., da un texto algo diferente, y presenta un pequeño esquema del apareto.

(14) A los Mss. indicados por BUBNOV, *op. cit.*, pág. 316, debemos añadir el ms. 225 de Ripoll fol. 21 *v*.,el ms. de Leyden, Scaliger 38 (siglo XI) fol. 46*v*., y el ms. de Roma, Vatic. Regin. 598 (siglo XI) fol. 119*r*., todos los cuales presentan este pasaje asociado con otros caps. de la *Geom. inc. auct.*

en dos distintas recensiones o versiones, de las cuales sólo una de ellas había merecido cierta atencíon, muy exigua, de los historiadores, mientras que la otra parace haber pasado hasta ahora desapercibida.

La primera versión sóla ha merecido, segun lo que parece, que CANTOR en su estudio *Die Römischen Agrimensoren* (15) la registrara en el apendice y diera, por cierto, una transcripción asaz incorrecta e ininteligible, a base del Ms. de Salzburgo, Bibl. Monast. S. Petri a. V 7 (siglo XII-XIII), fol. 101r. Como quiera que CANTOR creía que las fuentes de la Geometría de Gerberto — tanto de la parte legítima como de la parte hoy tenida ya por ilegítima — eran de exclusiva procedencia clásica, de aquí que en su estudio sobre los Agrimensores romanos tuviera muy en cuenta dicha obra. Y al encontrarse en el antedicho Ms. de Salzburgo, la Geometría de GERBERTO y la *Geometria incerti auctoris*, acompañadas de ciertos caps. relacionados con el contenido de aquellas, publicó algunos de estos caps. en el apéndice. De esta manera publicó el texto referente a nuestro cuadrante, y en cambio dejó sin publicar el texto relativo a la medición de grandes profundidades, que algunos folios más allá (fol. 106v.-107r.) subseguía en el mismo manuscrito.

Pero tenemos otros Mss. más antiguos que el de Salzburgo, que nos ofrecen seriados todos aquellos textos, y que por lo que respecta al pasaje de nuestro cuadrante nos dan una versión mucho más correcta que la dada por CANTOR. Tal es el caso del ya citado Ms. del British Museum, Old Royal 15 B IX (siglo XI), el cual después de darnos tratados matemático-astronómicos procedentes o supuestos de GERBERTO y HERMANN CONTRACTO, el texto de la *Geom. inc. auct.* con algunos aditamentos, y la correspondencia cruzada entre GERBERTO y ADELBOLDO DE UTRECHT, nos ofrece acto seguido (fol. 70v.) el texto sobre la medición de grandes profundidades, subseguido del pasaje relativo al cuadrante con cursor para la determinacion de la hora. A fin de tener una versión más inteligible que la dada por CANTOR, he aquí el texto de este pasaje segun el Ms. del Brit. Mus. :

Si diei horas scire desideras per astrolabii partem quartam, sicut possint sciri

(15) Leipzig, 1875. Pág. 225. A ello alude BUBNOV, *op. cit.*, pág. 314 sig.

uideas, intellectuque quodammodo acuto discernas, hic enim est inueniendi modus. Sume eam diuisam in XC partes, per quam sapientes habitabilis terre diuiserunt latitudinem, ac in primis, que sit illius terre latitudo, in qua hore inueniendae sint inquire, eique medium curriculi medietatem compone. Hoc facto postquam diei mensis quotus sit inuento, qui insignitus est in medio curriculo eidem diei inuento perpendiculum superpone, et in qua superiorum linea ipsum perpendiculum superueniet uideatur, ac eiusdem lineae superiori extremitati illud paruum medicurriculum, quod super perpendiculum currit, superducatur. Deinde, hoc sic constituto, eandem astrolabii quartam solis radiis oppone, ut per eius utraque foramina intus eant. His uero per utraque intus euntibus, perspice super quam lineam perpendiculum pendeat, ac super eandem ipsam paruulum medicurriculum in perpendiculo positum constitue, ut primitus eidem medicurriculo ita posito tunc horarum inferius positarum ordine cognito, super quam perpendiculum superiaceat uideatur, atque eam horam certam esse uideas super quam perpendiculum superiacere uideatur.

A dicho texto acompaña en el Ms. del Brit. Mus. una figura explicativa — que reproducimos — la cual coincide con el grabado que a base del Ms. de Salzburgo, publicó CANTOR en las láminas del final de su libro mencionado. Pero es lo cierto que si intentamos seguir el procedimiento de la operación de determinar la hora, explicado en el texto, a base de la figura que le acompaña, veremos que ésta no coincide, que le faltan elementos. En efecto, después de haber colocado el hilo de la plomada en el dia de la fecha de la operación, que figura en el cursor, dice el texto : « et in qua superiorum linea ipsum perpendiculum superueniet uideatur, ac eiusdem lineae superiori extremitati illud paruum medicurriculum, quod super perpendiculum currit, superducatur »; más adelante dice el mismo texto : « perspice super quam lineam perpendiculum pendeat, ac super eandem ipsam paruulum medicurriculum in perpendiculo positum constitue... ». Desde luego que en la figura no hay trazas siquiera de estas lineas a las que se refiere el texto, y además, por el proceso de la operación indicado, en ninguna manera aquellas lineas pueden ser las lineas horarias que vemos en el tipo corriente de cuadrante con cursor v. gr. el de Mestre ROBERT ANGLÉS (16) o en el descrito por RABÍ ÇAG en los Libros de Astronomía de ALFONSO EL SABIO (17). Dada la gran concisión de aquel texto y la deficiencia de la figura, se comprende que haya podido quedar este pasaje tanto tiempo en la sombra.

(16) Vid. pág. 4 de la obra citada de P. TANNERY.
(17) Vid. la fig. nº 1.

En cuanto a la otra versión del pasaje relativo al cuadrante con cursor—versión a la que llamaremos texto *B* —, creemos que ha pasado del todo desapercibida por los historiadores, siendo así que ella es mucho más fecunda que la anterior, según tendremos de ver. Se presenta en algunos Mss. alternando con proposiciones o capítulos de la *Geometria incerti auctoris*, y desde luego acompañando inmediatamente al capítulo relativo a la medición de grandes profundidades. Estos Mss., de la más alta antiguedad, son : El ms. 225 de Ripoll (siglo X), fol. 21r.-21v.; y los otros dos muy relacionados con él, en su primera parte, que son: Leyden, Scaliger 38 (siglo XI), fol. 46v., y Roma, Vaticana Reginensis 598 (siglo XI), fol. 119r. El texto de esta versión no es, sin embargo, más explícito que el anterior, y no se presenta, que sepamos, acompañado de figura explicativa. El procedimiento de la operación se ajusta del todo con el anterior y hasta la forma de exposición es análoga. Seguramente un texto es recensión del otro, quizá el texto *A* lo es respecto del texto *B*. He aquí dicho texto, muy uniforme en aquellos Mss :

Componitur orologium cum astrolabii quarta parte que XC. gradibus dinoscitur insignita. Cum quasi horas diei uolumus inuenire hac quam subponimus regulam oportet ita agere. Primo latitudo climatis inquiritur. Inuente medietas cursoris superponitur, deinde super praesentem diem de cuius horis est inuestigandum perpendiculum dimittitur, et linea cuius extremo superiacet bene perspicitur, cuius linee alteri extremo superponitur perpendiculum et loco perpendiculi qui linee superiacet superponitur paruissimum curriculum. Hac quadra sic composita si uolumus horas diei inuenire opponatur soli ut per utrumque foramen eius radius uideatur exire. Post hec cuius linee extremitas perpendiculo superiacet inspiciatur et ipsum perpendiculum per quadram tandiu uoluatur donec eius paruissimum curriculum ipsi eidem linee in quocumque acciderit superponatur, et post hec, ordo horarum qui scribitur cernatur et cui perpendiculum superiacuerit, ipsa hora diei esse dicatur.

Tenemos, en consecuencia, registrados hasta ahora, dos textos o versiones relativos al empleo del cuadrante con cursor para la determinación de la hora, y dichas versiones se nos presentan relacionadas con una cuestión geodésica de la *Geometria incerti auctoris*, y redactadas de una manera asaz concisa, sin dar ninguna referencia sobre el instrumento, como si supusieran a éste ya bien conocido. Si buena parte de las cuestiones de la primera parte de la *Geom. inc. auct.* vienen a ser aplicaciones geométricas o topográficas del astrolabio planisférico, y han sido como destacadas

de un tratado sobre el empleo (*utilitatibus*) de dicho aparato (18), no sería también nuestro pasaje relativo al cuadrante con cursor, destacado asimismo de un tratado especial relativo a dicho cuadrante? La analogía del caso no puede ser más favorable, pero se podría alegar que a diferencia de lo que pasa con el astrolabio planisférico, no se encuentran en los siglos X, XI y XII tratados especiales relativos al cuadrante con cursor, y que cuando en el siglo XIII se escriben tratados sobre el cuadrante con cursor (19), el aparato descrito no conviene del todo con aquel cuyo funcionamiento hemos visto.

Sin embargo, tenemos la satisfacción de poder afirmar que efectivamente desde el siglo X existían en la Europa cristiana tratados o versiones especiales del cuadrante con cursor, las cuales prontamente se difundieron y de ellas nos han quedado múltiples copias en Mss. de los siglos XI y XII; estas versiones nos habrán de revelar la verdadera forma de dicho cuadrante y nos permitirán ver la relación que mantiene con el otro cuadrante devenido clásico en el siglo XIII o sea con el *Quadrans uetus* o *communis*. Estas versiones, derivadas seguramente del árabe, debían contener según la pauta entonces corriente, las dos partes : 1ª Descripción del instrumento (*De mensura*), y 2ª Empleo del mismo (*De utilitatibus*). Pero lo mismo que en otras traducciones del árabe, relativas a otros instrumentos astronómicos v. gr. el astrolabio planisférico, los traductores, compiladores y copistas daban mucha mayor importancia a la parte sobre el empleo del aparato que a la de su construcción : aquélla era de necesidad mucho más inmediata y general que ésta. Por esto encontraremos aquellas versiones bastante avaras en la 1ª parte (*de mensura*) y ya más accesibles en la otra parte (*de utilitatibus*).

Es el ms. 225 de Ripoll que nos ha conservado en versión única —según lo que hemos podido averiguar—un pequeño resumen de un tratado relativo al cuadrante con cursor, que designaremos con la notación C. En su folio 35r. aparece el epígrafe : INCIPIUNT REGULÆ DE QUARTA PARTE ASTROLABII », y acto seguido se da en pocas líneas una sumaria descripción del cuadrante con cursor, por cierto

(18) En nuestro estudio aludido sobre historia de las ciencias en Cataluña, desarrollamos convenientemente estas ideas.

(19) Los más antiguos parecen ser los de JUAN DE SACROBOSCO, Mestre ROBERT ANGLÉS, RABÍ ÇAG, etc.

demasiado sumaria para poder tener una idea perfecta del instrumento. Hay que tener en cuanta el carácter misceláneo de dicho Ms., compilado quizá para finalidades escolares, en el cual se han seriado y extractado una gran serie de materiales, a menudo sin el debido orden, sin una corrección esmerada y con sobradas lagunas y deficiencias.

He aqui el boceto de descripción del aparato, que nos da dicho Ms. :

INCIPIUNT REGULE DE QUARTA PARTE ASTROLABII

In curriculo XII menses. In medio parti'o NADAIR solis inuenies perpendiculare quo notantur XC gradus et in eo quoddam paruissimum curriculum cum quo notantur linee horarum. In X[a] linea LX puncta ob umbram solis notato XC gradus in radice a quibus et clima dinoscitur et altitudo solis inuenitur. L linee in directum a quibus notatur cursus horarum.

Como se ha podido ver no es mucho lo que hemos adelantado en el conocimiento de la composición o estructura del cuadrante con cursor. Además, es muy posible que dicho pasaje ofrezca, junto a su gran concisión, algunas incorrecciones, sobretodo en lo tocante a los números expresados en cifras romanas, que lo hagan doblemente dificil. Pero de él desprendemos que había una linea graduada, dividida en 60 puntos a fin de medir la sombra del sol : « In X[a] linea LX puncta ob umbram solis... », es decir, que en vez del cuadrado de las sombras que aparece normalmente en el tipo de cuadrante *uetus*, (Vid la fig. I), sería un arco de un cuarto de circunferencia, la « Linea X[a] », el que serviría para la medición de la sombra. El empleo de dicho arco en vez del cuadrado de las sombras se encuentra regularmente en el astrolabio esférico o redondo v.gr. en el descrito por el citado RABÍ ÇAG en los *Libros del saber de Astronomía* de ALFONSO EL SABIO (20), y alguna que otra vez aparece tambien en el astrolabio planisférico (21) o en cuadrantes del tipo avanzado del *quadrans nouus* (22).

Otro dato nos da aquel pequeño pasaje, concerniente preci-

(20) Vol. II, pag. 143.

(21) H. SAUVAIRE y J. REY-PAILHADE, *Sur une mère d'astrolabe arabe du XIII siècle portant un calendrier perpétuel avec correspondance musulmane et chrétienne. Journal asiatique*, año 1893.

(22) W. H. MORLEY, *Description of an Arabic quadrant. Journal of the Asiatic Society*, año 1860, pág. 322.

samente a aquellas lineas que se distinguían — haciendo sus veces — de las seis lineas horarias típicas del *quadrans uetus* (23). Al hablarnos del índice o margarita que se desliza a lo largo del hilo de la plomada nos dice : « et in eo quoddam paruissimum curriculum cum quo notantur linee horarum», y luego : « L linee in directum a quibus notatur cursus horarum ». De modo que nos habla de un número considerable de lineas *rectas* con las cuales se nota el curso de las horas. Tenemos bien declarada, pues, la diferencia que hay entre nuestro tipo de cuadrante y el tipo del cuadrante *uetus*. Sin embargo, la disposición de aquellas rectas no resulta claramente expresada, ni tampoco se puede facilmente inducir de los usos o aplicaciones del cuadrante con cursor, que a renglón seguido explica el ms. 225 de Ripoll. Dejamos para más adelante el estudio de dichas aplicaciones — las cuales habremos de encontrarlas asimismo en otros mss.—, obligados como estamos ahora de perfeccionar nuestro conocimiento de la estructura de nuestro cuadrante con cursor.

Es lo cierto que en ningun otro ms. entre los muchos que hemos consultado, encontramos descripción alguna de nuestro cuadrante. Sólo en un texto misceláneo, atribuido sin una base cierta de fundamento, a HERMANN CONTRACTO, se halla un pasaje en el cual se da una descripción detallada de nuestro cuadrante con cursor y de su aplicación para la determinación de la hora. Nos referimos al cap. V : « De distributione quadrantis ita ut certas diei horas possis inuenire » del *Liber secundus de utilitatibus astrolabii*, impreso sucesivamente a nombre de HERMANN CONTRACTO por PEZ y MIGNE (24). No es aquí el lugar para hablar con detención de estos libros *Deutilitatibusastrolabii*, atribuidos al monje de Reichenau; bástenos decir que ya BUBNOV (25) probó de un modo evidente que el *Liber primus* no podía ser de ninguna manera de HERMANN, sinó que seguramente se había escrito anteriormente, quizá por GERBERTO, a nombre del cual aparece en algunos mss. (26). Nosotros, en nuestro estudio aludido referente a la

(23) Véase en la fig. I.

(24) *Patrol. Lat.*, vol. 143, col. 409.

(25) *Op. cit.*, pág. 109, nota I.

(26) Las justas observaciones de BUBNOV no han sido tenidas en cuenta por los historiadores y críticos posteriores, los cuales aún en nuestros dias siguen atribuyendo el tratado *De utilitatibus astrolabii* a HERMANN CONTRACTO, o bien,

historia de las ciencias matemáticas en Cataluña, creemos haber probado la verdadera génesis, no gerbertiana, de este célebre tratado del astrolabio, el cual ya aparece en el ms. 225 de Santa María de Ripoll.

En cuanto al llamado *Liber secundus*, desde luego que no es un tratado orgánico de astrolabio, sinó una simple yuxtaposición de materiales diversos relativos a la descripción de un reloj cilíndrico « *Demonstratio componendi cum conuertibili sciothero horologici uiatorum instrumenti* » (27), a la mensurabilidad de la tierra (28), descripción del cuadrante con cursor, y aún pasajes de la *Geometria incerti auctoris;* acaba con un texto, dislocado, relativo al trazado de las lineas azimutales del astrolabio (29). BUBNOV explica el modo como PEZ se vería inducido a atribuir estos dos libros a HERMANN CONTRACTO, que no fué otro que el aparecer en los Mss. acompañando frecuentemente al tratado verdaderamente legítimo de HERMANN : *De mensura astrolabii* (30).

Pues bien, en el cap. V de dicho *Liber secundus* se explica con bastante detalle la disposición de nuestro cuadrante, pero creemos que en el texto adoptado por PEZ y MIGNE hay bastantes incorrecciones de los copistas, sobre todo en los números — que estarían expresados seguramente en los mss. con letras romanas —, y

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con igual falta de base, atribuyen sendos tratados del mismo a GERBERTO y a HERMANN CONTRACTO.

(27) Sobre bibliografía de mss. relativos a este pasaje (alguno de ellos del s. X) vid. L. THORNDIKE : *Of the cylinder called the horologe of travellers en* ISIS Nº 40 (1929), pág. 51-52, y E. ZINNER, *Horologium Viatorum (Anmerkungen zu den Mitteilungen von Herrn L. Thorndike)* en ISIS, Nº 44 (1930), pág. 385-387.

(28) El texto del cap. II : « *De magnitudine ambitus uniuersi orbis.* Quamuis Ambrosii, Theodosii Macrobii auctoritate... » lo encontramos con muy pocas diferencias, y sin aparecer a nombre de HERMANN CONTRACTO, en un ms. del siglo XI : Londres, Brit. Mus. Old Royal, 15 B IX, fol. 76*r*., alternando con otros pasajes relativos al astrolabio y al cuadrante.

(29) El cap. último : « *Quomodo circuli asmut depingendi sunt* » seguramente está dislocado de un tratado *De mensura astrolabii*, muy probablemente del de HERMANN mismo, pues falta en el texto de la edición de MIGNE, la parte relativa al trazado de los azimutes. Un cotejo de los mss. que contienen la obra de HERMANN, *De mensura astrolabii*, permitiría seguramente establecer un texto más fidedigno que el dado por MIGNE.

(30) Vid. BUBNOV, *op. cit.*, pág. 109 nota I y pág. 113 nota 5. Que el mismo autor del prólogo puesto a la edición de MIGNE, *ibid.* col. 382, no estaba muy seguro de lo que decía, lo prueban sus palabras : « Qua de re forte melius alii censebunt », « Nobis in re obscura et perplexa id sectandum uidebatur quod planius et clarius esset. Nam uerba HERMANNI admodum obstant ».

además creemos que el mismo redactor o recensionador del pasaje cometería alguna incorrección al describer el aparato.

Damos a continuación la parte descriptiva, con las notas principales o correcciones que creemos hacer resaltar :

Circuli quadram in 90 per quinquepartita interualla in summitate diuide. Hinc cursorem, qui infra uoluitur, sic metire. Verso ad te quarae centro in sinistra parte subtrahe 24 de ipsis 90, et per cursorem firmiter in sua concauitate manentem regula manente in centro posita et in gradu uicesimo quarto lineam subtrahe. In dextra uero parte de ipsis 90, 19 subtrahe (31), et regula manente in centro et in decimo nono gradu, duc lineam per cursorem et utrinque eam abscide, et haec est longitudo cursoris.

Deinde circulum infra circulum secundum placitum, inter quem duo alii ducantur, in quibus per 6 diuisis duodecim horae notentur, et recte per disterminum horae 3 et 4, et 10, et 11 (32), regula in centro manente, et qui in quadragesimo quinto gradu ducatur linea, quae dicitur aequinoctialis. Tunc in ipsa aequinoctiali linea cursor in 5 diuidatur in parte sinistra; quarum 3 quae proximae sunt aequinoctiali lineae, in 4 diuidatur unaquaeque, et in 9 duae reliquae. Similiter faciendum est in dextera parte, et notentur illa spatiola (33).

Mensium uero discretio sic fiat : in parte dextera duobus relictis spatiolis Ianuario 6, Februario 5, Martio 6, Aprilii 6, Maio 6 concedantur; Iunio 4 ante solstitium, et hoc in superiori circulo (34). In inferiori uero circulo duae post solstitium particulae dentur Iunio; Iulio autem 6, Augusto 6, Septembri 6, Octobro 6, Nouembri 6, Decembri 4 ante solstitium, et hoc est in inferiori circulo. Post solstitium autem in superiore mensium circulo duae particulae, quae restant, Decembri tribuantur. Dehinc autem, ut supra dixi, Ianuario sex. Tunc polo uerso ad te ab ipso polo usque ad ultimum circulum in sinistra parte ultimam lineam in 30 diuide (35).

(31) Teniendo en cuenta que la anchura del cursor es doble de la oblicuidad de la eclíptica y que en todas las figuras de cuadrante que hemos visto, de ese tiempo: de JUAN DE SACROBOSCO, Mestre ROBERT ANGLÉS, RABÍ ÇAG, se marca una oblicuidad de 24°, creemos que la lectura 19 está equivocada, y que probablemente se habrá interpretado la grafía XXIIII por XVIIII.

(32) Seguramente mala lectura del ms.; debe corregirse : « X et II » por « IX et X ».

(33) Muy probablemente es incorrecta esta división del cursor, y desde luego sobrado concisa. Parece que en los dos arcos del cursor había 5 grandes divisiones, divididas en 4 espacios las tres centrales, y en 9 pegueños espacios las dos extremas : o sea, que por cada uno de los dos arcos había 30 espacios. A estas espacios se ajustaban los meses, según luego se dice en el texto.

(34) Es bastante concisa y creemos también bastante incorrecta esta manera de indicar el modo como se distribuyen los meses en los dos arcos correspondientes del cursor. Desde luego, que no todos los meses pueden tener la misma amplitud, la cual ha de disminuir de un modo progresivo desde el equinoccio hacia los solsticios. Véase la fig. I, la cual es correcta, mientras que la fig. 2ª ya se descuidó en este particular.

(35) Seguramente y a juzgar por lo que después dice, se divide en 30 partes el segmento de lado del cuadrante correspondiente a las pínulas, que está comprendido entre el vértice del cuadrante y el extremo del arco límite interior de

Dehinc circulum intimum de parte dextera a prima diuide usque ad ultimum horae sextae per 30 et sic a puncto in punctum duc lineas, quae dicuntur Almucantarath (36). Secundum hanc descriptionem quadra superius depingatur.

Así como los anteriores textos relativos al cuadrante con cursor pasaron desapercibidos a los historiadores, el pasaje atribuido a HERMANN CONTRACTO ya fué tenido en cuenta, si bien no con la atención que merecía. Así P. TANNERY en su citado estudio del tratado sobre el cuadrante (*Quadrans uetus*) de Mestre ROBERT ANGLÉS, dice : « La más antigua mención en el Occidente latino de un instrumento de este género (si bien que con un nombre algo diferente : *quadra astrolabii*) es probablemente la que se encuentra en el cap. V del libro II de la obra *De utilitatibus astrolabii* publicada por PEZ a nombre de HERMANNUS CONTRACTUS. Pero si este instrumento posee ya un cursor, el procedimiento para encontrar la hora descansa sobre un principio esencialmente diferente, a juzgar por la figura y por la descripción, desde luego bastante oscura » (37). A TANNERY han seguido los otros historiadores que se han ocupado de cuadrantes (38). Pero creemos poder afirmar que, por las palabras de TANNERY, parece que éste no se dió buena cuenta de la estructura y funcionamiento del cuadrante explicado en el tratado atribuido a HERMANN, pues llega a afirmar que se basa en un principio esencialmente diferente al del cuadrante *uetus*, a juzgar por la figura y la descripción del texto. Ello es bien explicable teniendo en cuenta que el texto es seguramente incorrecto, que la figura que aparece en la edición de MIGNE es del todo deficiente (39) y no ilustra para nada aquella

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las horas. Por lo que sigue en el texto, también se dividía en 30 partes el arco interior aludido, y se unían con lineas rectas unas y otras divisiones. Estas lineas rectas, verdaderas proyecciones de los puntos del arco interior sobre su radio—el segmento del lado del cuadrante—, son lo que caracteriza e individualiza nuestro tipo de cuadrante. Parece que el número de ellas no era constante en todos los casos. Así en el texto atribuido a HERMANN se habla de 30 lineas . en el ms. de Ripoll se consigna « L linee », y en la fig. 3ª aparecen 90 lineas rectas de proyección.

(36) La calificación de « Almucantarath » dada a las lineas de proyección del cuadrante, es a todas luces incorrecta, y prueba como el recensionador de este pasaje no estaba muy fuerte en técnica astronómica arábiga.

(37) *Op. cit.*, pág. 26 sig.

(38) A. ANTHIAUME y J. SOTTAS, *op. cit.*, pág. 73 sig. y R. T. GUNTHER, *Early Science in Oxford*, vol. II, pág. 158 sig. Oxford, 1923.

(39) La forman sólo los dos lados del cuadrante y algunos arcos concéntricos que deben corresponder a los límites del cursor.

descripción, y que los anteriores textos, que podían dar cierta luz, no son citados por TANNERY.

La sola descripción que aparece en el *liber secundus* atribuido a HERMANN, expurgada de sus incorrecciones, bastaría para tener una idea verdadera de nuestro cuadrante y de los principios que regulan su funcionamiento, pero, a mayor abundamiento, podemos tener la satisfacción de presentar a nuestros lectores una figura bastante esmerada del mismo (Vid. fig. 3ª), que hemos podido encontrar en el Ms. Vatic. Regin. 1661, siglo XI ex., fol. 86 v., exhornando unos pasajes sobre aplicaciones del cuadrante con cursor, relacionados con los ya aludidos del ms. 225 de Ripoll, y en los cuales nos vamos a fijar prontamente.

De la figura del cuadrante que nos ofrece el ms. Vatic. Regin. 1661, — la cual tan bien ilustra el texto atribuido a HERMANN CONTRACTO — se desprenden claramente las características de nuestro instrumento. Sin embargo, parece que dicha figura no está del todo correctamente trazada. Conviene con la figura del ms. del Brit. Mus. (fig. 2ª) en que la graduación del arco de altura está como invertida, pues la plomada cae en la graduación XC estando la visual dirigida al horizonte. En cuanto al cursor — en el cual tampoco aparecen los signos zodiacales relacionados con los meses — ofrece una anchura dudosa, y parece haber sido trazado incorrectamente. Desde luego que no es simétrico en sus dos partes extremas correspondientes a los solsticios. Y creemos que la división última de estos no la representan las dos líneas extremas del cursor, lineas exteriores a las palabras : « iunius » «december», sinó los extremos del arco de graduación interior del cursor, de modo que que el solsticio de verano corresponde a una oblicuidad de 23°, y el de invierno a una de 25° (40). En cuanto a las rectas de proyección, vemos que son en número de 90, correspondiendo 15 a cada una de las seis divisiones de las horas. Como ya dijimos anteriormente, este número de líneas de proyección no coincide con el señalado por el ms. de Ripoll : « L linee », ni tampoco con el numero que aparece en el texto, no bien seguro, a nombre de HERMANN CONTRACTO; más adelante veremos como en cuadrantes semejantes al nuestro o que por lo

(40) En general, la oblicuidad que figura en los cursores de este tiempo es de 24°.

menos cumplían funciones análogas, el numero corriente de lineas de proyección era de 60.

Estas lineas de proyección es lo que caracteriza nuestro tipo de cuadrante y lo diferencia del *quadrans uetus* o *communis* : las lineas horarias de éste han venido a reemplazar las 90 ó 60 lineas de proyección de aquél. Pero es lo cierto que el procedimiento de la operación para calcular la hora en los dos tipos de cuadrante es muy semejante — como se habrá podido notar a base del simple enunciado de los textos *A* y *B* —, y ello nos manifestará la relación que guardan aquellos diferentes trazados, y como es inadmisible la opinión citada de TANNERY de que los dos cuadrantes descansen en un principio esencialmente diferente.

Las lineas de proyección en el tipo de cuadrante que podríamos llamar *uetustissimus*, vienen a indicar las dos funciones trigonométricas del arco correspondiente, de modo que si admitimos como punto de origen de los sucesivos arcos que vaya marcando la plomada en el limbo graduado del cuadrante — graduación que nos indicará el arco de altura correspondiente a la cosa a la-cual dirigimos la visual —, la graduación marcada por la plomada con un arco de altura O, tendremos que las lineas de proyección correspondientes nos darán los cosenos de los arcos sucesivos, y la distancia que haya en el lado del cuadrante entre el pié de la linea de proyección correspondiente y el vértice del ángulo recto del cuadrante, nos dará el seno de aquel arco. En la fig. de la lámina 13 las lineas de proyección están trazadas de un modo deficiente pues dejan de ser verdaderas perpendiculares al otro lado del cuadrante. De todos modos, a medida que el arco aumenta, aumenta también el seno hasta hacerse igual al radio. Así es que el numero exacto de lineas de proyección viene a ser casi indiferente, pues sólo afecta a la aproximación mayor o menor de las divisiones del arco. Andando el tiempo, ya en pleno Renacimiento y Edad Moderna, y con fines puramente trigonométricos, estas divisiones llegaron a alcanzar un límite cada vez más aproximado.

Si nos fijamos ahora en el tipo del cuadrante *uetus*, por ejemplo en el dibujado por RABÍ ÇAG (lámina 11), veremos que la linea horaria de medio dia es una semicircunferencia que tiene por diámetro el radio del cuadrante interior límite de las seis lineas horarias, y que al dirigir una visual para tomar el arco de altura de alguna cosa, el segmento de hilo de plomada comprendido

entre el vértice del cuadrante y la semicircunferencia de la linea horaria de medio dia, nos indica también el seno del arco de altura tomado. También a medida que aumente el arco, el seno aumentará hasta hacerse igual al radio del cuadrante interior.

Tenemos, pues, que las dos disposiciones de los dos cuadrantes conspiran al mismo fin, o sea el de proporcionar el seno del arco de altura = Colatitud ± Declinación, dato que como veremos es esencial para la determinación de la hora. Y hallado este elemento, toda la técnica posterior de los dos cuadrantes es paralelemente igual.

Vamos, en consecuencia a ver las operaciones que se efectuaban con el cuadrante *uetustissimus*, teniendo buena cuenta de relacionarlas con las análogas del cuadrante *uetus*. Nos guiaremos por el articulado de los capítulos correspondientes que nos ofrecen el ms. 225 de Ripoll, fol. 35v.-37v.; el ms. 1661 de la Vaticana Reginensis, fol. 86r.-86v.; el ms. 15 B IX Old Royal del British Museum, fol. 70v.-71r.; el ms. 235 de Avranches, fol. 67v.-68r.; y el ms. 283 del Corpus Christi College de Oxford, fol. 105 v. Ya vimos antes que el capítulo sobre la determinación de la hora por medio del cuadrante *uetustissimus* se encontraba formando parte en algunos mss. del texto de la *Geom. inc. auct.* (41).

He aquí la primera aplicación de nuestro cuadrante, que nos ofrece el ms. 225 de Ripoll, fol. 35v. : Se trata de la determinación de la latitud del clima en que estamos :

Quando queris scire clima in quo sis prospice diligenter ante meridiem solis altitudinem ita ut per ambo foramina radii equaliter transeant, et interim dum manum tibi erigere necesse est scias nondum te peruenire ad meridiem et quando non est opus te manum leuare, scias te esse in meridie, et tunc illud perpendiculare caute notato, in quo gradu cadat, et postea illud curriculum hac et illac uolue, et diem presentem mensis illius in quo es, usque ad gradum admoue, et diligentissime prospice in quo gradu medietas illius curriculi remaneat, et a primordio graduum numero notato, usque ad medium curriculi, et cum illo quod occurrit inuenies numerum climatis in quo es.

Un texto análogo nos ofrecen los mss. Regin. Vatic. 1661, fol. 86r. y Avranches 235, fol. 67v.-68r. He aquí su transcripción :

De latitudine clymatis

Sumpta astrolabii quarta parte dum lucidus sol condescenderit medium axem

(41) Véase la pág. 222.

in cuiuslibet diei meridie, dum radius eius exierit per utraque foramina uideatur in quo graduum perpendiculum steterit de XC cui gradui presens dies presentis mensis in quo hoc agitur descriptus in curriculo adiungatur. Et alter gradus contra quem steterit linea diuidens martium et septembrem inspiciatur, et ab eo ceteri gradus retro numerentur, et quot fuerint, tota esse latitudo clymatis dicatur. Verbi gratia. Stat perpendiculum in XXIII gradu cui apponitur prima dies ianuarii in quo hoc agitur. Et que contra XXV gradum media linea diuidens martium et septembrem inuenitur, LIII latitudo climatis esse dicatur.

El procedimiento explicado en los anteriores textos es el mismo, y conviene tanto al tipo de cuadrante *uetus* como al *uetustissimus*. Hay que observar el sol al pasar por el meridiano, y la plomada nos indicará la altura meridiana, y sabiendo que la altura meridiana es igual a la colatitud del lugar, sumada la declinacion si esta es boreal o restada si es austral, tendremos que, poniendo en aquella graduación el dia del mes en que operamos tal como aparece marcado en el cursor, de modo que ambos coincidan, automáticamente habremos sumado o restado la declinación de aquel dia, y el numero de grados que haya entre la parte media del cursor y el otro extremo del cuadrante nos indicará la latitud buscada : « et alter gradus contra quem steterit linea diuidens martium et septembrem inspiciatur, et ab eo ceteri gradus retro numerentur, et quot fuerint tota esse latitudo clymatis dicatur».

Debemos decir que el ejemplo que aparece en el texto « De latitudo clymatis » de los mss. de Roma y Avranches, seguramente está equivocado, a causa de la mala lectura de las cifras por parte de los copistas; ya hemos visto que éstas no coinciden en los mss.

En el ms. 225 de Ripoll, un poco más adelante (fol. 37v.) y en los citados mss. de Roma y Avranches, así como en el del Brit. Mus. Old Royal 15 B IX, fol. 76 r. aparece otro cap. con el epígrafe « Item aliud theorema de eadem ratione » o bien con el titulo : « De altitudine clymatis percipienda », en el que se da otro procedimiento para la determinación de la latitud, el cual lo mismo conviene al cuadrante que al astrolabio; pero en aquellos últimos textos, a pesar de aparecer articulado con otras proposiciones de cuadrante, sólo se da con referencia al astrolabio. He aqui el texto del ms. de Ripoll :

Si queris scire in quo climate sis, accipe altitudinem solis in meridie in equinoctio, et uide quot gradus XC supersint ei altitudini. et tunc quere cui tabularum hic numerus inscriptus sit.

y el de los mss. de Roma, Londres y Avranches :

DE ALTITUDINE CLYMATIS PERCIPIENDA

Sumpto astrolapsu in meridie quo sol primum gradum arietis intrauerit, donec eius radius per utraque foramina mediclinii exierit, numerus graduum in dorso astrolabii intueatur, et diligenter intuitus de XC auferatur, et quanti uel quot superfuerint tota altitudo esse dicatur. Verbi gratia. Si superfuerint gradus in dorso astrolabii LX quibus sublatis de XC remanent XXX. Igitur XXX dicitur esse latitudo.

Como se ha visto, el procedimiento solo puede emplearse cuando el sol pasa por el punto vernal, primer grado de Aries, o tambien primer grado de Libra, pues no habiendo es estos dias declinación solar, el arco de la altura meridiana nos dará ya la colatitud, y restándola de 90 grados tendremos la latitud del lugar.

El ms. 225 de Ripoll después del pasaje sobre la determinación de la latitud, nos ofrece acto seguido el caso inverso (fol. 36r.), o sea, conocida la latitud del lugar, determinar la altura meridiana del dia de referencia. He aqui el texto :

Quando autem queris scire altitudinem solis in meridie, ex quo sciuisti diligenter clima in quo es, tandiu illud curriculum hac et illac moue, quousque medietatem illius numero climatis admoueas, et firmiter cum certo signaculo, quousque in illa terra fueris teneas, et ponas perpendiculare super presentem diem mensis in quo es, et uide super quem punctum perpendiculare ceciderit, illorum LX qui in X linea sunt notati, et qualis numerus perueniat punctorum, talis est altitudo umbre solis in meridie in illa terra in quo es, et bene memorie commendato, et scriptis ne memoriam excedat notato.

El procedimiento es el inverso del anterior : poniendo el punto central del cursor en la graduación del limbo correspondiente a la latitud del clima, al colocar el hilo de la plomada sobre el dia de referencia en el cursor, tendremos automáticamente restada o sumada la declinación, y el mismo hilo nos indicará en la graduación del limbo la altura meridiana de aquel dia.

Además parece que en el cuadrante a que se hace referencia en el texto del ms. de Ripoll había, segun ya notamos anteriormente (42), una linea — un arco — dividida en 60 « puntos », a fin de medir la sombra del sol. Dicha linea que estaba en correspondencia con la graduación del limbo, es la que se alude al final del anterior pasaje. En el tipo de cuadrante *uetus* aparece normalmente en vez

(42) Véase la pág. 228.

de aquella linea graduada, el cuadrado de las sombras (véase la lámina I), pero ya notamos que en algunos cuadrantes y astrolabios o azafeas, aparece aquella línea (43).

Después de los anteriores pasajes, el ms. 225 de Ripoll nos da otra aplicacion del cuadrante *uetustissimus* — la más importante de ellas —, o sea la determinación de la hora «temporal», a base del conocimiento de la latitud del lugar, punto este último que ya fué objeto de los primeros caps. Ya vimos (44) como en muchos mss. — incluso el de Ripoll — el pasaje de la determinación de la hora temporal aparecía como articulado con caps. de la *Geom. inc. auct.*, y anteriormente dimos dos versiones muy semejantes del mismo, anotadas com las letras *A* y *B*. Debemos notar que en los mss. de Roma 1661 Reg., fol. 86v. y Avranches 235, fol. 67v., el texto *B* aparece formando parte de toda una serie de proposiciones relativas al cuadrante, de modo que se corrobora la hipótesis apuntada por nosotros de que este pasaje fuera destacado de un tratado de cuadrante para formar parte de la *Geom. inc. auct.* Es hora ya de que expliquemos el procedimento seguido en nuestro cuadrante, puesto que ya tenemos todos los elementos de juicio para conocer su estructura y aun sus relaciones con el cuadrante *uetus*. El ms. de Ripoll (fol. 36v.-37r.) inserta una explicación — texto *C* — que, conviniendo con los textos *A* y *B*, los amplía un poco. Después de los pasajes correspondientes a los anteriores problemas, dice :

Postea caute computa perpendiculare in qua remaneat linea, et caput illius linee diligenter infra tene, et ex altera parte supra similiter prouide. Perpendiculare autem usque ad illud superius caput admoue, et illud paruissimum curriculum de quo superius diximus super caput linee pone, et tandiu sinas in eodem loco quousque uideas non mutari umbram altitudinis umbre solis, quamdudum repperisti, et hoc facias uel VI aut VII^{em} diebus. Quando hoc habebis totum factum, et uis scire quot hore sint, uel que hora diei, oppone sinistrum humerum contra solem, et tandiu hac et illac manum erige uel equaliter tene donec sol utrumque pertranseat foramen, et uide in quo puncto illorum supradictorum LX perpendiculare cadat. Et quot inueniuntur puncta, talis est altitudo solis umbre illius hore quam inuenisti, et uide in qua linee sumitate stet perpendiculare, et tandiu hac et illac moue perpendiculare, quousque illud suum paruissimum curriculum in aliqua parte tangat eandem lineam. Et scias in qua parte steterit, in ascensu usque ad meridiem, et in descensu usque ad XII ^{am} presentem te inuenisse horam (45).

(43) Véase la pág. 229.

(44) Véase la pág. 222.

(45) He aqui el texto explicativo correspondiente en el pasaje (texto D) : « De

El proceso de la determinación de la hora por medio del cuadrante *uetustissimus* es sensiblemente el mismo en los cuatro textos *A*, *B*, *C*, *D*, que hemos transcrito. Conocida la latitud del lugar y por medio de ella la altura meridiana del sol en el dia de referencia — materia objecto de los anteriores caps. —, se nota cuidadosamente la linea de proyección que por su parte más inferior corta el hilo de la plomada, y se hace resbalar este hilo a lo largo de aquella linea hasta llegar a su otro extremo superior y la longitud de hilo que se halle comprendida entre el vértice del cuadrante y el extremo superior de la linea de proyección será un dato importante con el que habremos de operar; un indice « curriculum » o una perla « margarita » que se mueva a lo largo del hilo de la plomada nos lo indicará. Este dato nos revela—segun desprendimos anteriormente de la figura de la lám. 13 — el seno del arco de la altura meridiana observada. Así es que llamando *a* el radio—de longitud arbitraria—del cuadrante interior límite de las lineas de proyección, y designando por *h* la altura meridiana, tendremos que la longitud de hilo marcada por el índice será *a* sen. *h*. Esta longitud tendrá que ser el radio del arco que el índice o perla describirá paralelemente al arco diurno en las sucesivas horas del dia, la determinación de las cuales nos proponemos averiguar.

distributione quadrantis ita ut certas diei horas possis inuenire » atribuido a HERMANN CONTRACTO : « Inuenturus horam cum quadra, que est cum cursore, aequinoctialem lineam cursoris quae Martium Septembremque discriminat, duc super numerum, qui in gradibus in umbone notis altitudinem eius climatis, in quo est, praesentant, ut puta, si sis in nostro, id est in septimo climate, pone praedictam lineam super 48, ab imo sursus incipiens computare. Locato sic cursore duc perpendiculum super praesentem diem, in qua horam inquiris, et uide quam lineam ex Almucantarath a sinistro uersum cursorem inferius tangat. Qua notata duc perpendiculum ad caput eiusdem lineae ex altera parte uersus pinulas, et ubi eam tangit perpendiculum, tu in cera perpendiculi fac punctum, uel perpendiculum, ut apud quosdam mos est, habeat cursorem paruulum. Hunc tu uolue ad locum, ubi praedictam lineam tangat perpendiculum. Hac ratione inuento puncto, et in cera per circulum perpendiculi notato, ubi altitudinem solis inuenta poscit ratio. Quam inuenturus leuata quadra sinistrum soli humerum oppone, et tandiu eam huc illuc uerte, donec sol utrumque pinnularum foramen penetret recto tenore. Gradus, quem tunc perpendiculum in umbone tetigerit, tibi, quantum sol iam ascendit, descendit, liquido demonstrabit. Super eumdem gradum perpendiculo iacente uide quam lineam ex Almucantarath superius tangat regionem. Super hanc lineam tu punctum in cera super pendiculum factum uel circulum pendiculi, ubicumque eam tangere possit, ponens considera, qui horae perpendiculum superiaceat, et haec est, quam (quantus) tuus labor exquirat. »

En efecto, luego de determinado el seno de la altura meridiana, dice el texto que tomaremos la altura del sol en el momento de referencia, haciendo que sus rayos pasen a través de las dos pínulas de mira, y nos fijaremos en qué linea de proyección corta por su extremo inferior el hilo de la plomada; una vez que hayamos fijado bien esta linea, haremos tambien resbalar el hilo de la plomada por ella hasta que el índice o perla, mantenido en la misma posición anteriormente fijada, alcance aquella línea de proyección, y la posición que entonces el hilo de la plomada ocupe en la graduación de las horas del cuadrante interior, nos indicará la hora y parte de hora buscada : « et uide in qua linee summitate stet perpendiculare et tandiu hac et illac moue perpendiculare quousque illud suum paruissimum curriculum in aliqua parte tangat eandem lineam. Et scias in qua parte steterit in ascensu usque ad meridiem et in descensu usque ad XII presentem te inuenire horam » (Texto *C*); « Post hec cuius linee extremitas perpendiculo superiacet inspiciatur et ipsum perpendiculum per quadram tandiu uoluatur donec eius paruissimum curriculum ipsi eidem linee in quocumque acciderit superponatur et post hec, ordo horarum qui scribitur cernatur et cui perpendiculum superiacuerit, ipsa hora diei esse dicatur » (Texto *B*).

Es claro que cuanto mayor haya sido el valor de *a sen. h* = seno de la altura meridiana de aquel dia, más abajo en el hilo de la plomada se encontrará el índice, y por tanto, menos habrá de girar la plomada para que el índice encuentre la linea de proyección correspondiente a la elevación del sol en aquella hora; al ser, pues, el arco diurno mayor, nos encontraremos en una hora más baja, o sea más cercana a la de prima o duodécima. Lo contrario pasaría con un valor inferior de *a sen. h*, o sea con una menor altura meridiana.

Vayamos a ver ahora la relación que guarde el procedimiento de la determinación de la hora en el cuadrante *uetustissimus* con el seguido en el cuadrante *uetus* : veremos como no solamente no cabe la diferencia esencial de principios de que nos hablaba TANNERY en un alusión al cuadrante descrito a nombre de HERMANN CONTRACTO, sinó que, en rigor, el procedimiente es casi el mismo.

Ya vimos como la semicircunferencia interior que corresponde a la linea horaria de mediodía, nos daba también el seno del arco de la altura meridiana. En cuanto a las otras lineas horarias (Cfr.

Lámina 11): la Va o VIIa, IVa o VIIIa; IIIa o IX, IIa o Xa, y Ia o XIa, son arcos que pasan por las divisiones equidistantes del cuadrante interior en : 75°, 60°, 45°, 30°, 15° (46). El centro de cada uno de dichos arcos está en la perpendicular bajada en el punto medio de las cuerdas que sustentan aquellos arcos en su intersección con el lado de las pínulas del cuadrante o bien con su prolongación. De modo que el radio de los distintos arcos horarios aumenta en la siguiente proporción : Radio de la linea horaria Va o VIIa : 1/2 secante 15°; radio de la linea horaria IVa o VIII : 1/2 secante 30°, etc.

Si seguimos la operación de determinar la hora tal como nos la describen RABÍ ÇAG (47) o Mestre ROBERT (48), veremos que, una vez determinado el seno de la altura meridiana del dia de referencia y puesto el índice sobre la longitud correspondiente del hilo de la plomada — igualmente que como hicimos con el cuadrante *uetustissimus* —, tomaremos por las pínulas de mira la altura del sol en el momento u hora que queremos determinar, y nos fijaremos sobre qué linea horaria cae entonces la perla o índice, y esta linea nos indicará la hora buscada. Si cae entre dos lineas horarias, la proximidad de la perla respecto una u otra de las lineas nos indicará las fracciones de la hora. Claro está que, lo mismo que en el cuadrante *vetustissimus*, cuanto más abajo esté la perla — por ser mayor el seno de la altura meridiana — mayor será el arco diurno y, por tanto, a igual altura observada estaremos más cerca de la hora de prima o de duodécima.

Vemos, pues, que el procedimiento de los dos cuadrantes para la determinación de la hora es sensiblemente igual, si bien en el cuadrante *uetus* se ha simplificado la disposición sustituyendo el trazado de las lineas de proyección por el de las lineas horarias, que nos dan los mismos datos trigonométricos, con una mayor facilidad y rapidez de inspección.

Esta identidad hace que el cuadrante *uetustissimus* participe de

(46) En el cuadrante *uetustissimus* tambien se halla el cuarto de círculo interior, límite de las lineas de proyección, dividido en seis arcos de 15° cada uno, correspondientes a las seis horas (cf. lám. III).

(47) Capítulo V de la 2ª parte del « Libro del cuadrante » : « De saber las oras temporales passadas del dia en que estas ».

(48) « Ad inuentionem hore per quadrantem cum cursore », pág. 55 de la edición citada de TANNERY.

los mismos defectos que los atribuidos al cuadrante *uetus* por DELAMBRE (49) y TANNERY (50). El cuarto de circunferencia interior, límite de las lineas de proyección en el cuadrante *uetustissimus* y límite de las lineas horarias en el cuadrante *uetus*, dividido en seis partes de 15° correspondientes a las horas « temporales » I^{a}-VI^{a} o VII^{a}-XII^{a}, no representa exactamente el arco semidiurno sinó en los dos dias equinocciales, en los cuales la hora temporal vale los 15°. De modo que en los demás dias no era bien exacto y cuanto mayor fuera la declinación y la latitud, mayor será el error. En los paises musulmanes, de poca latitud, el error no era considerable y a esto se debería la propagación del cuadrante con cursor.

Además, el cursor establece una correspondencia fija entre la marcha del sol en el zodíaco y los dias del año civil, lo cual es inexacto pues por el fenómeno de precesión de los equinoccios, el punto Aries avanza progresivamente hacia los meses de Febrero y Enero. Esto se podía atenuar cambiando de cursor--segun TANNERY un cursor podía servir durante la vida de un hombre si no se exigia una aproximación superior a 2/3 de grado — o bien empleando tablas de declinacóin y longitud constantemente rectificadas.

Otro empleo del cuadrante *uetustissimus* nos ofrecen los mss. citados de Roma, Vatic. Regin. 1661, fol. 86v. ; Avranches 235, fol. 68r. y Oxford, Corp. Christ. Coll 283, fol. 105v. Con el título : « Ad altum cum quadra astrolabsus inueniendum (51) » nos describen un empleo geométrico del cuadrante para determinar la altura de un objecto. He aquí el texto segun los dos primeros mss.

DE ALTITUDINE ET LATITUDINE ALICUIUS REI

Si uis cum quadra astrolabii latitudinem inuenire, dimitte perpendiculum per gradum XLV, et post hec tamdiu a te retro et ante pergatur, donec eiusdem rei cacumen per utraque eiusdem quadre foramina cernatur, et post hoc spatium inter te et radicem rei metiende addita tua statura mensuretur, et quot pedum ipsum spacium fuerit, tot pedum et altitudo rei metiende erit.

El procedimiento seguido es bien simple : Se hace girar el cua-

(49) *Op. cit.*, pág. 245.

(50) *Op. cit.*, pág. 28.

(51) El ms. de Avranches inserta el epígrafe : « De altitudine et latitudine alicuius rei »; el ms. de Oxford ofrece una redacción del texto algo diferente.

drante hasta que el hilo de la plomada coincida con la graduación 45 del limbo, y una vez en esta posición y sin mover el instrumento avanzaremos o retrocederemos del sitio en que primeramente estábamos hasta que a través de las pínulas de mira veamos el vértice o cima de la cosa cuya altura buscábamos. Logrado esto, la distancia que nos separe del pie (vertical) de aquella cosa, será igual a su altura, sumada pero a aquella la distancia respecto del suelo a que hayamos tenido levantado el instrumento. En efecto, la altura de la cosa — descontada la distancia al suelo a que está el cuadrante —, la distancia al pie de la misma y la visual dirigida a través de las pínulas con un ángulo de 45° al vértice de la cosa, forman un triangulo rectángulo isósceles, que nos da resuelto el problema.

Desde luego que este procedimiento exclusivamente geométrico no es privativo del cuadrante con cursor, sinó que puede emplearse el simple cuadrante sin cursor, o también se puede hacer uso de uno de los cuadrantes que figuran en el astrolabio, generalmente el inferior derecha; además, con el *quadratus geometricus* o con un simple triángulo rectángulo isósceles se puede operar, haciendo que la visual coincida con la diagonal o hipotenusa. El ms. 225 de Ripoll fol. 101r.-102r. nos ofrece un pasaje, quizá relacionado con los anteriores, en el cual se nos habla de esta aplicación del cuadrante del astrolabio y del *quadratus geometricus*(52). Recordemos, además, que en el cuerpo de la *Geometria incerti auctoris* se emplean normalmente estos procedimientos (53); precisamente su primer capítulo (cap. 16 del libro III) nos ofrece el anterior procedimiento, seguido en el cuadrante del astrolabio planisférico.

Tenemos, pues, explicada la estructura y funcionamiento de un cuadrante con cursor — que llamamos *uetustissimus* —, muy parecidos, por no decir iguales, a los del cuadrante *uetus*, y ello a base de mss. el más antiguo de los cuales, el 225 de Ripoll, remonta al siglo X (54). Además, téngase en cuenta que este ms. no es sinó un misceláneo a base de múltiples copias de distintos materiales, compuesto probablemente para usos escolares, y con

(52) Sobre la técnica de éste, véase la citada obre de WEISSENBORN, *Gerbert. Beiträge zur Kenntnis der Mathematik des Mittelalters*, cap. II, pág. 104 sig.

(53) Véase la pág. 221.

(54) Véase R. BEER, *Die Handschriften des Klosters Santa Maria de Ripoll*, pág. 46 de la traducción catalana. Barcelona 1910.

notorio descuido, con muchas lagunas y mutilaciones. Todos los materiales que se han compilado en este ms. son de materia astronómica, y la mayor parte de ellos son de procedencia arábiga. En el estudio sobre la historia de las ciencias físicas y matemáticas en Cataluña, a que hemos aludido anteriormente, ventilamos cuidadosamente los problemas de identificación y corelación de aquellos diferentes materiales : tratados de construcción y uso del astrolabio, tratados de relojes, proposiciones de medición geodésica, pasajes cosmográficos de BEDA, etc. Bástenos decir aquí que creemos que la fuente muy probable de aquellos tratados de astrolabio son las obras sobre el mismo tema de MACHALA, autor, probablemente judío egipcio, que floreció entre los años 770-820. ¿Sería también MACHALA el autor del tratado de nuestro cuadrante, la traducción latina del cual nos han conservado aquellos mss. de los siglos X y XI? Nos reservamos para más adelante resolver este importante problema, bastándonos por ahora recalcar la presencia en la Europa latina de tratados independientes relativos a nuestro cuadrante : « Regule de quarta parte astrolabii» (55), de indudable procedencia arábiga; el cuadrante con cursor que en ellos aparece tiene el trazado algo distinto del cuadrante *uetus*, pero obedece a los mismos principios y a la misma técnica y procedimiento. De modo que avanzamos en más de dos siglos la introducción del cuadrante con cursor en Europa. Seguramente el mismo traductor o traductores que tradujeron aquellos tratados de astrolabio y relojes que figuran en el Ms. 225 de Ripoll, tradujeron nuestro tratado de cuadrante, precisamente en el tiempo en que la España musulmana, bajo la égida de príncipes tan eminentes como ABDERRAHMÁN III y ALHÁQUEM II, se esforzaba por importar y asimilarse la ciencia greco-arábiga que con tantos fulgores lucía en el islam oriental. Recordando que GERBERTO — espíritu tan ansioso de ciencia — estudió matemáticas y música en Cataluña, que hacia el año 984 dirige desde Reims una carta a su íntimo amigo « Lupito barchinonensi », pidiéndole el libro de astronomía que ha traducido (del árabe) « Librum de astrologia translatum a te mihi petenti dirige et si quid mei uoles in recompensationem indubitate re-

(55) Así se desprende sin duda alguna, del ms. 225 de Ripoll, y de un modo probable, a base de los citados mss. de la Vatic. Regin. 1661, y de Avranches 238.

posce » (56), no está fuera de lo probable asociar el nombre Lupitus Barchinonensis a la obra de traducción de aquellos tratados de astrolabio y del nuestro sobre el cuadrante. Lo corrobora el hecho de aparecer precisamente en este tiempo en Barcelona, la figura de un eminente clérigo de aquel nombre, distinguido por condes y obispos, arcediano de Barcelona y luego, según parece, abad del monasterio de Santa María de Arles, el cual era « omni sciencia literari pleniter instructo » (57). Tenemos aún composiciones suyas sobre asuntos religiosos, en las cuales se distingue el mismo gusto por un estilo clasicizante — era la tónica que dominaba entonces en la escuela de Ripoll — que en los prólogos o introducciones a aquellos tratados de astrolabio (58).

En el citado ms. 225 de Ripoll, fol. 37v., luego del pasaje sobre la determinación de la hora, se incluye una correspondencia de los siete climas y de la gradación con que disminuye la diferencia de un clima a otro.

Así VIII° es la diferencia entre el clima I y el II; VII° es la diferencia entre el clima II y el III; VI° es la diferencia entre el clima III y el IV, etc. (59). Luego sigue un pasaje, ya visto, sobre la determinación de la latitud del clima, observando el sol al mediodía de uno de los dias equinocciales (60) y, por fin, se cierra el tratado con este a manera de colofón : « CCCCL. anni iam transacti sunt ex quo iste liber compositus est : tunc Almucatil in Piscibus nunc in Scorpione ». En el ms. siguen luego largos excerpta de BEDA, para reaparecer, ya hacia el último tercio del ms., nuevas recensiones — mas que traducciones — de originales arábigos. Recuérdese que ya hemos destacado el especial carácter misceláneo que presenta el ms. 225 de Ripoll. Pues bien, ¿a qué original corresponde este colofón : al del tratado sobre cuadrante, sólo, o bien al de toda una obra astronómica de la cual procedieran los distintos materiales

(56) Véase el texto completo en BUBNOV, *op. cit.*, pág. 101.

(57) Véase VILLANUEVA, *Viaje literario*, vol. XII, apénd. III.

(58) Véase el texto de un sermón de LLOBETÓLUPITUS, publicado por L. NICOLAU d'OLWER, *Escriptors llatins de Catalunya* a *Estudis Universitaris Catalans*, vol. XIV, pág. 70 segs. Barcelona, 1929.

(59) Sobre otros ejemplos de un aumento proporciomal en la latitud de los climas, véase E. HONIGMANN, *Die sieben Klimata und die πόλεις ἐπίσημοι. Eine Untersuchung zur Geschichte der Geographie und Astrologie im Altertum und Mittelalter*, pág. 101 sig. y 184 sig. Heidelberg, 1929.

(60) Véase la pág. 236.

del ms. 225 de Ripoll, entre los cuales aparece, más o menos extractado y recensionado, el tratadito de nuestro cuadrante? No podemos dilucidarlo, pero confesamos que no nos inclinamos a creer que aquel colofón sea privativo del texto sobre cuadrante. Desde luego que el original del cual deriva la recensión latina del ms. 225 de Ripoll, es un original árabe; la presencia de la palabra *Almucatil*, o sea la denominación del planeta Saturno, típica de la terminología astronómica de los musulmanes occidentales (61), lo permite afirmar; además, en el tratado del cuadrante del ms. 225 Rip. hay otras palabras árabes. Pero a su vez, este original árabe podía ser traducción o derivación de otra fuente, pues dice : « CCCCL anni iam transacti sunt ex quo iste liber compositus est : tunc Almucatil in Piscibus nunc in Scorpione ». Si este explicit fuera traducción fiel y exacta — lo que creemos probable — del explicit que figuraba en el original árabe, de modo que el que habla sea el autor árabe, por la expresión « iste liber » habrá que entender su fuente, pues, teniendo en cuenta el lapso de 450 años transcurridos, no podía ser una obra en árabe. En efecto si el ms. 225 de Ripoll es de la última mitad del siglo X, y él, a su vez, es un miscelláneo que copia y extracta otros materiales latinos, podemos colocar la traducción latina de la fuente árabe, un poco anterior, entre mediados y el último tercio del s. X, y, por lo tanto, suponiendo reciente o sea hecha en el mismo s. X la obra árabe, aquel « iste liber » redactado 450 años antes, no podia ser obra árabe, sinó que habría de ser o siríaca o índica o, más problablemente, griega.

Aun suponiendo que el explicit sea original del traductor latino — el cual no sería anterior al segundo tercio del siglo X —, nos encontramos con los mismos resultados que en la hipótesis anterior. Cabe objetar un error del copista en las letras romanas que expresan aquella fecha, cosa que, como hemos visto, tan frecuente es en los mss. tratándose de números romanos. En efecto, computando a base de las tablas de ALBATTANI (62) el movimiento en longitud de Saturno : 356º 8′ en 30 años árabes, resulta que en el espacio de

(61) Cf. DOZY, *Supplément aux dictionnaires arabes*, vol. II, pág. 308, a base del *Glosario latino-arabigo español del s. XI*, editado por C. F. SEYBOLD, Berlin, 1900, y H. SUTER, *Die astronomischen Tafeln des Muhammed ibn Musa Al-Khwarizmi in der Bearbeitung des Maslama ibn Ahmed Al-Madjriti und der latein. Uebersetzung des Athelhard von Bath*, pág. 245. Copenhague, 1914.

(62) Edición de A. C. NALLINO, vol. III, pág. 25. Milan, 1907.

450 años — ya árabes ya cristianos — no podía experimentar aquella variación indicada en el colofón. Pero hemos de confesar que, aún aceptando alguna incorrección en las letras romanas expresivas de aquella cantidad de años, parece que se trataba de un número reiterado de centenas, o sea que había algunas *C* repetidas, de modo que poniéndonos en el caso de mayor modernidad para el colofón : que fuese redactado por el traductor latino, y que no pasase de dos centenos con algunos unidades aquella fecha, tendremos que hemos de colocar el original hacia principios del siglo VIII, o sea antes del período de florecimiento de la cultura árabe.

El explicit anterior solo nos prueba, pues, como la obra latina deriva de una traducción árabe, la cual asimismo puede proceder de otra fuente pre-árabe. No sabemos más, y aún no podemos decir con seguridad si aquel explicit es privativo o no del tratadito de cuadrante con cursor que nos ofrece el ms. 225 de Ripoll.

Es hora ya — para terminar nuestro estudio — que veamos los antecedentes seguros o posibles, de nuestro cuadrante, y ver si por ellos podemos encontrar su filiación, ya que no hemos podido hacerlo con aquel explicit. Confesamos, pero, que en esto no ofrecemos nuestras conclusiones como definitivas, sino como coadyuvantes a la solución del problema.

Para el estudio de los instrumentos astronómicos de los árabes, en particular de los cuadrantes, tenemos la obra de L. Am. Sédillot « *Mémoire sur les instruments astronomiques des arabes* » (63), hecha a base de la gran información de técnica de aparatos de astronomía y gnomónica que nos transmitió el astrónomo marroquí Abulhasán Ali ben Omar (siglo XIII); recientemente la bibliografía se ha enriquecido con la obra de P. Schmalzl : « *Zur Geschichte des Quadranten bei den Arabern* » (64), hecha a base de fuentes manuscritas y de los trabajos de la escuela de E. Wiedemann, K. Schoy, I. Frank, etc. En estas obras podemos seguir facilmente la marcha de la técnica de los cuadrantes árabes, tan variados y múltiples. Pues bien, hemos de decir que un cuadrante igual al « uetustissimus » y aún al « uetus », no lo encontramos entre los distintos tipos de cuadrantes árabes presentados, pero, en cambio, podemos

(63) París, 1844. (Ext. del tomo 1° de « *Mémoires présentés par divers savants à l'Académie royale des Inscriptions et Belles-Lettres, et publiés par cette Académie* ».

(64) München, 1929.

encontrar sus elementos en combinación con otros varios, en algun cuadrante.

La presencia de las lineas de proyección de los grados del arco de altura sobre uno de los lados del cuadrante — generalmente en número de 60 o 90 — a fin de determinar los senos y cosenos de los distintos arcos tomados, es el elemento típico del llamado cuadrante *dastur* o *quadrans canonis*, que solia formar la segunda faz del cuadrante usual entre los árabes, llamado por antonomasia « cuarto de circulo » (65). El trazado de estas lineas de proyección se amplió gradualmente, bajándose aquellas líneas desde los grados del arco de altura a los dos lados del cuadrante, y siendo atravesadas por radios que iban desde los grados del arco al vértice del cuadrante, por diagonales que cruzaban los pequeños cuadrados formados por este cañamazo, etc.

En la obra de SCHMALZL pueden verse estos sucesivos desenvolvimientos del trazado trigonométrico del cuadrante *dastur* de los árabes (66); y el mismo desenvolvimiento tuvo lugar entre los cristianos (67). Pero todos estos cuadrantes se presentan sin cursor. Sólo en un cuadrante de un preclaro autor hispano-árabe se ofrece el cursor acompañando a un tipo de cuadrante *dastur* : es el cuadrante que ABULHASÁN atribuye a AZARQUIEL (68), pero en él hay algunos aditamentos y alguna pequeña diferencia que disimulan la analogía. Además, el texto de ABULHASÁN, explicativo de la estructura y técnica del cuadrante de AZARQUIEL, al decir de SÉDILLOT, es bastante confuso, y la figura que presenta no está bien puntualizada (69). Por la lámina que reproducimos, se ve que

(65) SÉDILLOT, *op. cit.*, pág. 82 y seg.; SCHMALZL, *op. cit.*, pág. 72 seg. Este último autor cree que la figura de cuadrante *dastur*, explicada por ABULHASÁN y comentada por SÉDILLOT, no es bien completa, pues sólo ofrece las lineas de proyección desde el arco a uno de los lados del cuadrante, siendo así que en obras posteriores sobre dicho cuadrante figuran aquellas lineas en doble proyección a los dos lados del cuadrante, acompañadas de lineas radiales que van desde el vértice del cuadrante al arco de altura. Pero muy posiblemente estas adiciones son posteriores desenvolvimientos, y téngase en cuenta lo que el mismo ABULHASÁN nos dice (Apud SÉDILLOT, *op. cit.*, pág. 106, n. 1) sobre la semejanza del cuadrante *dastur* con el de AZARQUIEL — sobre el que hablaremos — y con el cuadrante *dahir* de la azafea del mismo AZARQUIEL.

(66) *Cf.* págs. 83 sigs.

(67) *Cf.* la obra citada de SOTTAS y ANTHIAUME, pág. 105 sigs.

(68) SÉDILLOT, *op. cit.*, pág. 104 sigs.; SCHMALZL, *op. cit.*, pág. 124.

(69) Dice SÉDILLOT, pág. 106, n. 2 : « Toute cette partie du ms. 1148 était

el cursor es exterior al arco de altura, variación que es muy indiferente : en cambio, a lo largo del lado del cuadrante que corresponde a la linea meridiana, podía moverse, en una ranura al efecto excavada un pequeño cursor o clavija, del cual colgaba un hilo de plomada con el cual se determinaban distintos problemas astronómicos : declinación de las estrellas, el seno verso de su arco diurno, su azimut, etc. Además, desde el punto del lado del cuadrante, origen de la graduación del arco de altura, tomado como polo, se bajan arcos que tocan el otro lado del cuadrante y lo dividen en 90 partes iguales. El mismo ABULHASÁN reconoce el parentesco que el cuadrante de AZARQUIEL guarda con el cuadrante *dastur* y con el mismo cuadrante que figura en el dorso de la azafea de AZARQUIEL (70). Se ve, pues, que el insigne astrónomo del rey ALMAMÚN DE TOLEDO, acreció con algun pequeño aditamento un tipo de cuadrante *dastur*, con cursor, que es igual al que nosotros hemos llamado *uetustissimus*, y en uso ya entre los cristianos del siglo X. No podemos precisar más respecto a la filiación de nuestro cuadrante, y hemos de decir que la adición del cursor al simple cuadrante *dastur*, muy probablemente no se hizo esperar mucho desde que los árabes emplearon el tipo manual de cuadrante, con fines astronómicos, pues que con ello se dispensaban del uso de unas tablas de declinación. Pero las noticias referentes al cuadrante son muy escasas entre los autores árabes de estos primeros siglos. Es ALHUARIZMI quien en su obra MAFATIH-AL-ULUM « Las llaves de las ciencias », nos habla del uso del cuadrante para la determinación de la altura y de las horas (71). Pero hasta ABEN YUNUS († 1009) no tenemos una noticia concreta de los cuadrantes manuales que empleaban los musulmanes. Al hablarmos del problema de la determinación de la alquibla (72), nos describe el « cuadrante de senos » — del mismo tipo que el cuadrante *dastur* —

très difficile à traduire, quoique la matière en paraisse assez claire à présent, parce que la rédaction d'ABOUL-HASSAN est fort confuse, que la plupart des lettres manquent sur la figure du ms. et que quelques-unes sont mises dans le texte l'une pour l'autre ». Véase la lámina 14, que hemos reproducido de la obra de SÉDILLOT.

(70) Dice ABULHASÁN : « Por lo que respecta al cuadrante de AZARQUIEL, se relaciona con el cuadrante *dastur* y con el cuadrante *dahir* de la azafea de AZARQUIEL. » Apud SÉDILLOT, *op. cit.*, pág. 106, n. 1. Véase nuestra nota 65.

(71) Véase SCHMALZL, *op. cit.*, pág. 31.

(72) Véase SCHMALZL, *ibid.*, pág. 33.

como un instrumento muy conocido y de uso general. De modo que — dice SCHMALZL — no nos equivocaríamos al suponer su uso ya muy extendido en el siglo X, el siglo áureo de los musulmanes de Oriente. Y, como hemos dicho, el cursor no podía tardar a asociarse al dicho cuadrante *dastur*. La presencia del cuadrante *uetustissimus* en mss. latinos del s. X, procedentes de traducciones árabes, es una prueba palmaria de ello.

Una duda se presentará ahora al lector : ¿ Cómo el cuadrante *uetustissimus* — correspondiente al *dastur* — fué desplazado por el cuadrante *uetus* en el cual las lineas horarias han sustituido las lineas de proyección ? Pues en los mss. ya conocidos de los siglos X y XI, y en el texto atribuido a HERMANN CONTRACTO, sólo se habla del tipo de cuadrante *uetustissimus*, y, en cambio, a principios del siglo XIII ya parece el *quadrans uetus* bastante conocido, de modo que Mestre ROBERT no se nos ofrece ya como inventor suyo, y es más, un autor algo anterior suyo, GUILLELMUS ANGLICUS al escribir en 1231 en Montpeller o Marsella, un tratado sobre la azafea, y descibirnos el cuadrante inferior de la derecha del dorso del instrumento, nos dice : « In quarta inferiori que est a dexteris, lineetur quadrans sine cursore. Designantur hore e contrario ei quadranti qui annulum siue perpendiculum habet, quia mouetur instrumentum, hic mouetur regula »; « Deinde lineentur hore secundum doctrinam datam de quadrante, tamen, ut dixi, e contrario ei quadranti qui mouetur, etc. » (73). El mismo tipo del *quadrans uetus* aparece en otros autores del siglo XIII : JUAN DE SACROBOSCO, CAMPANUS DE NOVARA, RABÍ ÇAG, etc. Pero este tipo de cuadrante, llamado *uetus* por oposición al *nouus* de PROFIAT TIBBON, es llamado en los mss. más antiguos : « *quadrans secundum modernos* » (74), y esta denominación que tanto estrañaba a TANNERY creemos que muy probablemente corresponde a la sustitución del cuadrante *uetustissimus* por el *uetus*, o sea, a la sustitución de las lineas de proyección por las lineas horarias, las cuales permitían una observación más clara y por tanto más rápida. Lo que es curioso es que al mandar ALFONSO EL SABIO a RABÍ ÇAG que compusiera la primera parte del « cuadrante para rectificar » —

(73) Véase el texto, a base del ms. 7195 de la Bibl. Nat. de Paris, en SÉDILLOT, *op. cit.*, pág. 186, y las correcciones hechas por TANNERY, *op. cit.*, pág. 75.

(74) Véase TANNERY, *op. cit.*, pág. 21.

puesto que de ella, a diferencia de la segunda parte : « Sobre el uso del dicho cuadrante », no se encontraba a la sazón —, este autor nos describiera el tipo de cuadrante *uetus*, sin hacer la más mínima referencia al cuadrante de AZARQUIEL, con el cual, segun hemos visto, tanta relación mantenía. Se ve que en la propria corte de ALFONSO EL SABIO, donde tanta veneración se sentía por la obra del gran astrónomo toledano, en alguna cosa dejaba de seguírsele.

Pero, segun parece, los autores latinos occidentales llegaron a más, pues que en la propria azafea de AZARQUIEL introdujeron aquella sustitución, o sea que en el cuadrante inferior de la derecha del dorso de la azafea reemplazaron las lineas de proyección — « Las linnas de la ordenacion » de la traducción mandada hacer por ALFONSO X — por las lineas horarias. Asi en el tratado que aparece en el ms. 7195 de la Bibl. Nat. de Paris : « Incipit compositio tabule que saphea dicitur siue astrolabium ARZACHELIS (fol. 74r.-77v.), atribuido al citado GUILLELMUS ANGLICUS — seguido por el « Liber operationis tabule que nominatur saphea patris YSAAC ARZACHELIS » (fol. 78r.) traducido por PROFIAT TIBBON y JUAN DE BRIXIA en Montpellier, el año 1263 —, ya aparecen referencias a la estructura del *quadrans uetus* (75), y a ellos acompaña una figura de azafea de AZARQUIEL con aquella modificación. Esto y otras circunstancias hizo que RICO y SINOBAS (76) calificara esas traducciones de « dolosas », y creyera que estaban hechas, mas que a base del texto auténtico de AZARQUIEL, a base de alguna azafea o instrumento análogo que de España les hubuese llegado; STEINSCHNEIDER en sus « *Etudes zur Zarkali* » (77) argumenta contra la suposición

(75) Anteriormente ya anotamos algunas alusiones en la primera parte de la obra de GUILLELMUS ANGLICUS; en la segunda parte, editada por TANNERY, *op. cit.*, pág. 75 sigs., véase la sig. : « Longitudinem solis a medio celi per partes horarum gradus Solis diuide, et habes horam presentem et partem hore. Vel, si limitaueris horas in dorso instrumenti, ut dixi, eas inuenire poteris, cum signata fuerit altitudo solis meridiana in regula ut fieri solet in filo perpendiculi ». En la tradución de P. TIBBON y J. DE BRIXIA no hay alusiones a ningun cuadrante, a no ser que así se considerara la frase algo confusa relativa al dorso de la azafea (fol. 78v.) : « Deinde sequitur *quadrans aut duas umbras* ».

(76) *Op. cit.*, vol. III, pág. XXV sigs.

(77) Roma, 1884, pág. 34 sigs (Extr. del *Bullettino di Bibliografia e di Storia delle Scienze matematiche e fisiche*, vol. XIV-XVI). Véase tambien su gran obra : *Die hebraeischen Uebersetzungen des Mittelalters und die Juden als Dolmetscher*, pág. 590, Berlin, 1893.

de RICO, que no hay ningun signo de falsificación, pues en cuanto a la obra atribuida a G. ANGLICUS parece ser un resumen, y en cuanto a la traducción de P. TIBBON y JUAN DE BRIXIA, concuerda mucho más con los textos árabe, hebreo, que no el texto de la traducción que figura en las obras del Rey Sabio, y parece seguro que en éstas hay bastantes amplificaciones. Nosotros hemos compulsado el texto de la traducción del alfaquí Don ABRAHAM en la edición de RICO y SINOBAS, con los textos árabe, hebreo y latino de la obra de AZARQUIEL, y hemos llegado a consecuencias distintas de las de STEINSCHNEIDER, y que acreditan la gran fidelidad de la traducción de Don ABRAHAM, ordenada por el Rey Sabio. En una monografia especial que contribuirá a la ilustración de la bibliografia de AZARQUIEL, desarrollaremos convenientemente esta cuestión.

De modo que el texto de la azafea de AZARQUIEL que se nos ofrece en la traducción de Don ABRAHAM, hemos de considerarlo absolutamente concorde con la fuente árabe, y como quiera que al hablarnos del dorso de la azafea, nos describe la estructura y funcionamiento de un cuadrante con lineas de proyección, igual al trazado del *quadrans uetustissimus*, hemos de considerar como una innovación el tipo de cuadrante *uetus* que nos presenta G. ANGLICUS en su tratado de la azafea (Véanse láms. 15 y 16).

En corroboración de esto, hemos de decir que también hemos visto alguna azafea árabe, la cual reproduce exactamente el modelo descrito en la traducción ordenada por el Rey Sabio. Creemos, pues, que se obraría paralelamente, tanto en el cuadrante manual como en el de la azafea, la sustitución que diferencia el cuadrante *uetustissimus* del *uetus*.

Estas pequeñas variaciones que en los textos operaban los sucesivos recensionadores y copistas, nos han de servir de piedra de toque para verificar la posible autenticidad de unos pasajes que nos interesan, antes de dar por terminado nuestro estudio. Repetimos aquí que no es nuestro propósito dar por absolutamente definitivo lo que apuntemos. Anteriormente hemos visto — siguiendo a SCHMALZL — la escasez de fuentes concretas árabes relativas al cuadrante hasta ABEN YUNUS, el cual nos informa de que en su tiempo el cuadrante con cursor era de uso general : ALHUARIZMÍ (s. IX) nos había dado la otra única noticia, diciendo que el cuadrante era igual a un cuarto de círculo, y que con él

se determinaban la altura y las horas (78). Pues bien, tenemos un texto de un autor anterior a estos dos autores, que, de ser auténtico, nos daría una magnífica referencia del cuadrante.

Nos referimos al tratado de astrolabio de MACHALA, que, segun creemos haber probado en nuestro aludido trabajo, era bien conocido en la España musulmana y cristiana del siglo X. De este tratado desconocemos el texto árabe original, así es que hemos de referirnos a la traducción latina que de dicha obra nos ha quedado. No sabemos a qué traductor debemos esta obra, y no parece solida la creencia de WUSTENFELD de que fué JUAN HISPALENSIS este traductor (79). Pero esta misma traducción latina entraña algunos problemas de fijación y crítica textual, que han escapado a los historiadores, y que el mismo STEINSCHNEIDER no estuvo muy afortunado con ellos. Tenemos, en primer lugar, el texto de la edición de G. REISCH en su célebre M*argarita Philosophica* (80), el cual es bastante diferente del texto recientemente editado por R. T. GUNTHER (81), a base del ms. Cambridge, Univ. Libr. I i. 3, 3, del año 1276, y con notas y variantes a base del ms. algo posterior Ashmole 1796. Dice GUNTHER : A casual inspection, however, shows that the text of MESSAHALLA as printed by REISCH, is not the text of the Cambridge and Ashmole mss. It is true that the earliers chapters are common to both, but the REISCH text then proceeds to different topics and is fas shorter than the English Universities' texts. And as the latter probably represent more nearly the version wich CHAUCER studied, no further apology is needed for its extraction from the dust of ages ». Pero basta una simple lectura de la obra edición de GUNTHER para ver que tampoco su texto refleja una versión genuina y exacta de la obra de MACHALA. Lo denuncia la presencia de nombres de personajes seguramente posteriores a MACHALA, como ALBATEGNIUS, ALMEON (82), la presencia de una « Tabula stellarum fixarum uerificatarum per armillas Parisius » (fol. 71r.); además, en la parte II : « De operatione astrolabii » hay cierto desorden en el

(78) *Op. cit.*, pág. 31.

(79) *Cf.* M. STEINSCHNEIDER, *Die europäischen Übersetzungen aus dem Arabischen bis Mitte des 17. Jahrhunderts*, B, pág. 35.

(80) Basilea, 1583 (tercera ed.).

(81) Oxford, 1929.

(82) Véase la pág. 202 de la ed. de GUNTHER.

enlace de los capítulos : así después de haber hablado del modo de hallar las horas iguales o desiguales transcurridas (caps. 6-8), habla de la conversión de unas horas en otras (cap. 9) — lo que, en general, suele terminar la parte de texto destinada a horas —, y, luego, en los caps. 11 y 12, vuelve a hablar del modo de hallar las horas, ya por la alidada horaria o por las lineas horarias.

Precisamente este cap. 12 es el que nos interesa, y después volveremos sobre él.

Además, creemos que existen otros especímenes o recensiones de la obra de MACHALA. Anteriormente ya dijimos que, a nuestro juicio, los tratados de astrolabio — construcción y uso del mismo — que se guardan en el ms. 225 de Ripoll (siglo X) y en otros mss. de distintas bibliotecas de Europa, de los siglos XI y XII, derivan de la obra de MACHALA, bien conocida entre los musulmanes españoles del s. X. Hemos de decir aún que creemos haber hallado otro espécimen de la traducción de la obra de MACHALA. Nos referimos al tratado sobre construcción y uso del astrolabio que empieza : « Astrologicae speculationis exercitium habere uolentibus eius instrumenti ratio naturali quodam ordine primitus occurrit inuestiganda... ». Este texto, que STEINSCHNEIDER coloca como de autor y traductor anónimo (83) lo hemos estudiado a base del ms. 10009 de la Biblioteca Nacional de Madrid (siglo XIV), procedente del fondo de la Biblioteca de la Catedral de Toledo, fol. 18r.b.-23r. b. y de él hemos desprendido que es una recensión de la obra de MACHALA. En la parte primera : « De composicione astrolabii » discrepa bastante de los textos editados de MACHALA, si bien hay pasajes idénticos; en cambio, se parece ya más a alguna las traducciones que aparecen en aquellos mss. de los siglos X y XI, é incluye la misma lista de estrellas con la posición correspondiente.

En cuanto a la segunda parte : « De operatione astrolabii » — también colocada como de autor y traductor anónimo por STEINSCHNEIDER (84)—, ya es casi igual a la edición de REISCH y GUNTHER, si bien ofrece algunas omisiones. Pensamos publicar algun dia este texto de MACHALA, y entonces podremos puntualizar más lo que decimos. Por ahora, bástenos saber la falta de fijeza con que se nos presentan las traducciones de la obra de MACHALA, y, por

(83) *Die europ. Übers. aus d. Arab.*, B, pág. 59.
(84) *Ibid.*, pág. 59 y 60.

tanto, la falta de un criterio seguro para ponderar la autenticidad de tal o cual pasaje. Establecido esto, vamos a ver ahora los pasajes que nos importan por su relación con el cuadrante.

En el cap. 3 de la primera parte se habla del cuadrado de las sombras que figura en el dorso del astrolabio, y después de explicar su estructura corriente, dice : « Et si uis in astrolabio 2 quadrantes habere, facias in alia quarte septemtrionali scilicet et orientali que est iuxta eam, similiter. Potes etiam inter quadrantes 2 eosdem infra circulum mensium constituere lineas horarias ut fit in quadrante, per quas eciam horas diei naturales in dorso, ut patet in figuris suprahabitis ». En efecto, en el fol. 62r. del ms. de Cambridge, reproducido por GUNTHER, aparece el dorso de un astrolabio, con doble cuadrante de las sombras, junto con cada uno de los cuales aparece el trazado de las lineas horarias, como en el cuadrante *uetus*.

En el cap. 12 de la parte segunda, ya aludido anteriormente por nosotros, se habla del modo de hallar la hora por estas lineas horarias del dorso del astrolabio; hay que tener en cuenta que ya en los caps. 3 y 6 se había hablado del modo de hallar las horas desiguales, por los medios corrientes en los antiguos tratados de astrolabio, o sea por la faz del astrolabio y por medio del arco diurno transcurrido. Pues bien, he aqui como empieza el texto del cap. XII : « Item per alidadam in dorso et lineas horarum inter latera gnomonis, si sint posite ut in quadrante, sic ». Tenemos, pues, otra referencia al cuadrante con un trazado del tipo *uetus*. La operacion es igual a la ya conocida, haciéndose una señal con tinta en la alidada que hará las veces del índice o perla (85).

Al final de la segunda parte en la edición de REISCH (86), después de haber hablado de las aplicaciones geodésicas del astrolabio, se dice : « Possunt et profunda metiri, uelut dictum est in praxi quadrantum ». En la edición de GUNTHER falta esta alusión última, la cual, pero, no nos dice si se refiere al cuadrante de las sombras del propio astrolabio, con el que se efectuaban aquellas operaciones geodésicas, o bien a otro cuadrante independiente.

(85) Creemos que este pasaje nos puede explicar la técnica seguida con la alidada de la azafea de AZARQUIEL, y como no tiene una base bien segura la hipótesis, algo complicada, que sugiere TANNERY, *op. cit.*, pág. 73 y 74, recogida por GUNTHER: *Early Science in Oxford*, vol. II, pág. 201, para obviar la falta de cursor en la alidada.

(86) *Cf.* STEINSCHNEIDER, *Die arab. Lit. der Juden*, pág. 18.

Tenemos, pues, en el texto latino de la obra de MACHALA dos alusiones al cuadrante con las seis lineas horarias, pero las dos nos hablan de dicho cuadrante de un modo muy accidental : sólo en el caso de que estuviere dibujado en el dorso del astrolabio. Además, el primero de los dos pasajes aparece al final de un capítulo, como algo adicionado en último lugar, y el segundo pasaje no guarda un orden bien normal en la sucesión de los caps. ¿Serán los dos pasajes, auténticos o bien algo añadido por algun copista recensionador en la obra de MACHALA, cosa que ocurría bien a menudo? Nosotros no podemos solucionar este problema, y sólo nos contentamos con apuntarlo, esperando que otros puedan verificar de un modo definitivo la autenticidad o inautenticidad de esta precoz alusión al cuadrante, tipo *uetus*. Séanos, pero, permitido apuntar nuestra impresión contraria a la autenticidad de aquellos dos pasajes. En primer lugar, en ninguno de los tratados de astrolabio primitivos — derivados, algunos de ellos, del de MACHALA, segun creemos haber probado (87) — : de ALI BEN ISA, MASLAMA, ABEN SAFAR, ABUSALT DE DENIA, encontramos el trazado de las lineas horarias en el dorso del astrolabio junto con el cuadrado de las sombras; tampoco las encontramos en aquellas traducciones latinas del s. X y XI; y revisando la serie de admirables fotograbados de astrolabios que nos presenta el proprio GUNTHER en el Vol. II de su « Early science in Oxford », vemos que es con el transcurso del tiempo que en el dorso del astrolabio se asocian diversos trazados junto con el del cuadrado de las sombras v. gr. : el de las seis lineas horarias, el de las lineas de proyección, el de los dos semicírculos para hallar los senos y cosenos, el de los seis arcos concéntricos representativos de la posición de los signos del zodíaco. Véanse los astrolabios árabes de Selden (año 1224), el de Laud (año 1400), y otros latinos de plena Edad moderna.

Además en el texto de MACHALA que nos ofrece el ms. 10009 de la Bibl. Nac. de Madrid no aparece el cap. XII de la segunda parte, ni ninguna alusión a aquel tipo de cuadrante.

Si la obra de MACHALA sobre construcción y uso del astrolabio era bien conocida en la España musulmana y cristiana del siglo X, y en ella se aludía y describía un tipo de cuadrante dotado del

(87) En nuestro trabajo sobre la historia de las ciencias físicas y matemáticas en Cataluña, el volumen 1º del cual es de reciente aparición.

trazado de las seis lineas horarias, como en el trazado del cuadrante *uetus*, ¿ es verosímil que los recensionadores de la obra de MACHALA: MASLAMA, ABEN SAFAR, las traducciones latinas de Ripoll, no recogieran aquella feliz disposición? ¿ Y que el celebrado constructor de astrolabios IBRAHIM BEN SAID ASAHLÍ de Valencia (siglo XI) no la introdujera en sus admirables instrumentos (88)? ¿ Y que el autor de las traducciones y recensiones que figuran en el ms. 225 de Ripoll y demás mss. citados de los siglos XI y XII, nos describiera el tipo del cuadrante *uetustissimus*, que tiene un trazado que obliga a una técnica más lenta y pesada que no la del cuadrante *uetus*? ¿ No nos habrían dado alguna alusión a este último?

Estas son las sugestiones que nos permitimos presentar y que, a nuestro juicio, nos impiden aceptar aquellos pasajes del texto de MACHALA como inconcusamente auténticos y, por tanto, como precedentes que expliquen y filien el tipo del cuadrante *uetustissimus*.

(Madrid) J. MILLÀS VALLICROSA.

(88) De él se guardan aun algunos astrolabios, de los cuales sólo ha sido bien estudiado hasta la fecha, el del Museo Arqueológico Nacional de Madrid; cf. E. SAAVEDRA, *Astrolabios árabes* a *Museo Español de Antigüedades*, vol. VI, pág. 295 sig.

A propos de notations mathématiques

Ce serait une tâche vaine et vraisemblablement peu utile que de vouloir dès à présent compléter le récent ouvrage de CAJORI sur les notations mathématiques, mais il peut être intéressant cependant d'illustrer par des exemples nouveaux les conclusions du regretté professeur. Cette brève note a pour objet d'insister sur un point qui risque, à cause de la masse énorme des faits, de ne pas apparaître dans une lumière suffisante au lecteur de *A History of mathematical notations* (1), savoir l'orgie de symboles mathématiques dont furent témoins la fin du seizième siècle et le dix-septième siècle, chez les auteurs secondaires surtout, et qui eut une extension à peine concevable.

Ouvrons le petit traité qui a pour titre : De Numeris Geometricis. OF THE NATURE and proprieties of geometricall numbers. First written by LAZARUS SCHONERUS, and now Englished, enlarged and illustrated... by THOMAS BEDWELL Esquire... London 1614 (2). Nous y trouverons le cas sans doute unique de trois notations différentes au sein d'un seul ouvrage et pourvues d'une signification identique. En effet, l'auteur pour indiquer l'addition fait successivement usage des signes suivants : 1) $\times$: «36 $\times$ 64, that is, 36 added to 64, do make for the summe 100 (p. 13); 2) $\rtimes$: « Now 1440 $\rtimes$ 64, or the summe of 1440 and 64, is 1504 » (p. 18) ; 3) $+$: «6 $+$ 1, that is 7» (p. 23).

Il vaut la peine de noter que le but tout pratique de ce manuel était d'enseigner l'usage d'un instrument, le «mesolabium architectonicum».

J. PELSENEER.

(1) Vol. I, 1928; vol. II, 1929; The Open Court Publishing Company, Chicago. *Isis*, 12, 332-336; 13, 129-130.

(2) En dépit du titre, ce quarto semble dû en réalité au neveu de THOMAS BEDWELL († 1595), WILLIAM BEDWELL († 1632), le père des études arabes en Angleterre.

Notes and Correspondence

1. Queries.

Query No. 18.—Dr. ESMOND R. LONG, Professor in the Department of Pathology of the University of Chicago (*Isis*, 12, 436; 15, 490), is planning to write a biography of ANTONIO BENIVIENI and will appreciate any information on the subject.

Query No. 19.—The undersigned desires information concerning the following documents :

1. Letters of ALOYS DAVID, Director of the Prague Observatory, to Freiherr F. DE ZACH, of Gotha, from 1791 to 1805 (59 ZACH's letters are in the archives of the Observatory of Prague and are ready for publication).

2. Letters addressed to ZACH by the Royal Society of Sciences of Prague (1797-1799) and signed by one of these scholars : A. STRNAD, Med. Dr. JOHANN MAYER, Abbé GRUBER, Prof. F. GERSTNER.

3. ZACH's manuscript (66 pages) entitled « Vertheidigung » or « Défense » against the R. Soc. of Sciences of Prague, in his controversy (1797) on the chronometer of JOSIAH EMERY.

4. Letters of ALOYS DAVID to the Director of the Observatory of Vienna, P. FR. TRIESNECKER (1798-1816) and to H. C. SCHUMACHER in Altona (1825-1830) concerning the same chronometer.

The undersigned takes the liberty to ask the Directors of the Archives and Libraries to give him a notice in the case of existence of these documents in their institutions. As regards the Observatory of Gotha, the Library and the Lindenau-Museum of Altenburg and the Observatory and Library of the University of Vienna, the result of his investigations was negative.

Dr. OTTO SEYDL, *Astronomer*
of the National Observatory Prague I, Clementinum.

2. Miscellanea.

Ghubār numerals. — While reading " The origin of the Ghubār

numerals, or the Arabian abacus and the articuli," by Dr. SOLOMON GANDZ (*Isis*, 16, 393-424) the following facts were recalled to my mind.

1. p. 407. Was ALCUIN the first to use the word *articuli*? Cf. these two passages in BEDE's *De temporum ratione* (MIGNE, *Patrologia latina* XC cols. 297-8; col. 505). In the first chapter, after discussing at some length the old system of finger-reckoning in which he does *not* use the word *articuli*, he says, "Est et alterius modi computus, *articulatim* decurrens: qui, quoniam specialiter ad Paschae rationem pertinet, cum ad hanc ex ordine ventum fuerit, opportunius explicabitur. Potest autem et de ipso quem praenotavi computo quaedam manualis loquela, tam ingenii exercendi quam ludi agendi gratia figurari: qua litteris quis singillatim expressis verba, quae iisdem litteris contineantur, alteri qui hanc quoque noverit industriam, tametsi procul posito legenda atque intelligenda contradat, vel necessaria quaeque per haec occultius innuendo significans, vel imperitos quosque quasi divinando deludens. Cujus ordo ludi vel loquelae talis est: cum primam alphabeti litteram intimare cupis, unum manu teneto: cum secundam duo, cum tertiam, tria. Et sic ex ordine caeteras."

Further on, (chapter 55) in the midst of a discussion of the method of finding Easter by means of the 19 year cycle, he continues, "Memoratu autem dignum videtur quia quidam ob compendium calculandi utriusque ordinem circuli, et solaris videlicet et lunaris, transferunt in *articulos;* nam quia manus humana *articulos* habet, adjunctis unguibus, X et IX, singulis his singulos aptantes annos, lunarem cursum in laeva manu intrinsecus a radice pollicis incipiunt, et in ungue minimi digiti intrinsecus eundem consummant. Item quia manus binae *articulos*, exceptis unguibus, habent XXVIII, iis singulos annos singulis aptant, inchoantes a minimo laevae digito, et in dexterae pollice complentes, non ut in lunari cyclo singulos ex ordine digitos expedientes ad numerum, sed ob rationem quadrantis per quaternos transversim digitos quadriennium omne signantes, ita ut minimorum bis terni *articuli* digitorum totidem bissextiles contineant annos; item proximorum a minimis bis terni *articuli* digitorum proximos a bis ternis bissextis annos totidem explicent, secundi similiter secundos, et tertii digiti tertios totidem annos aequa ratione complectantur; porro septimus bissextilis cum tribus se sequentibus annis bis binos sibi pollicum vindicet *articulos*. Hoc sive alio quisque sibi calculator ordinare voluerit modo, nihilominus circulum utriusque sideris libenter capient manus. Sed innumera hujusce disciplinae, sicut et caeterarum artium, melius vivae vocis alloquio quam stili signantis traduntur officio."

2. p. 410. That England was in touch with Greeks and Syrians even before the time of ALCUIN can be illustrated by the biographies of THEODORE OF TARSUS and BENEDICT BISCOP.

THEODORE OF TARSUS, having been ordained by VITALIAN in 668, became archbishop of Canterbury in the following year, at the age of 67. He was " learned in secular no less than in ecclesiastical philosophy, and thought in both languages, Greek that is and Latin " (BEDE, *Historia ecclesiastica*, IV, 1). Although educated in Athens, THEODORE was a native of Cilicia and in spite of the fact that it is not stated at what age he left Tarsus, it is possible that as a young man he might have come into contact with some of the Nestorians of that region " who like SEVERUS SEBOKHT were familiar with the Hindu numerals as early as 662 " (GANDZ, p. 411). After reaching England THEODORE and HADRIAN " did instruct their hearers in the sciences of metric, astronomy and the science of ecclesiastical numbers " (BEDE, *op. cit.*, IV, 2).

BEDE, in his *Vita abbatum* tells of the various trips BENEDICT BISCOP, the founder of Jarrow and one of BEDE's teachers, made to Rome and Gaul for books and other objects for his monastery. On one of his trips he " brought back (from Rome) many books on all subjects of divine learning... On his way home he stopped in Vienne, and recovered of the friends to whom he had entrusted them the books he had bought." This visit to Vienne shows that there was some connection between England and the East at this period. From Roman times on through the middle ages, this city had been a great center for Syrian and Greek merchants (cf. the inscriptions in C. I. L. XII, 217 ff., and ALLMER, *Inscriptions antiques de Vienne*, 5 vols.). According to M. BRÉHIER, in the sixth and seventh centuries, Vienne was in very close touch with Syria and Greece (« Les colonies d'Orientaux en Occident », in *Byzantinische Zeitschrift* XII, 11 ff.). As a result of the barbarian invasions, the foreign merchants and scholars were now preserving their own languages and customs even more carefully than they had in the earlier centuries.

3. p. 395. The fact that Lorraine " in the eleventh century was the chief centre for the study of the abacus and produced such eminent mathematicians as HERIGER OF LOBBES " etc. (HASKINS, *Studies in mediaeval science*, 334) should be noted in connection with the information that HERIGER OF LOBBES, as well as ABBO OF FLEURY and GERBERT knew the Ghubār numerals. If, as BUBNOV thinks, these numerals came from the ancient Roman-Greek symbols used on the abacus, and if these symbols had been used in Italy in the period of or posterior to BOETHIUS, might they not have reached Lotharingia earlier than the time of GERBERT? For an important contact between Italy and Lotharingia in the tenth century see the account of the Italian journey of JOHN OF GORZ as given by Prof. J. W. THOMPSON in the " Introduction of Arabic science into Lorraine in the tenth century," (*Isis*, 12, 184ff.). On the other hand

if, as WOEPCKE has said, the Ghubār numerals had reached Spain through the Neo-Pythagoreans by c. 450 A.D., they might still have traveled north through the agency of this same JOHN OF GORZE who went to Cordova in 953, which would help to explain their early presence in this region. JOHN was in Cordova, which was under Arabic control, for about three years. In the tenth century there was regular trade between Verdun and Spain (cf. LUITPRAND, bishop of Cremona 920-972, *Antapodosis*, VI, 6), and merchants would be interested in systems of counting, and thus the numerals might have reached Lotharingia by this route.

4. For an interesting example of the use of *articuli* for the word *ten* see the passage on leap-years by ROBERT GROSSETESTE in his *Compotus*, chapter I, " Dico ergo quod centenarius et millenarius et omnis numerus aggregatus ex istis, ut 200 et 300 et 2000 et 3000 et consimiles, sunt bissextiles; et de numeris intermediis inter aggregatos ex centenariis et millenariis, dico quod de digitis sunt bissextiles numeri 4 et 8. De *articulis* autem omnis *articulus* denominatus a numero pari ut 20 et 40 et 60 et deinceps. De numeris compositis omnis numerus compositus ex *articulo* numeri inparis et binario vel senario ut 32 et 36. Et omnis compositus ex *articulo* numeri paris et quaternario vel octonario, ut 24 et 28. Hii autem omnes numeri et soli sunt numeri bisextiles, quia hii omnes et soli proveniunt ex multiplicatione quaternarii."

Harvard Library 189 MARY CATHERINE WELBORN.
Cambridge, Mass.

Critical edition of the collected papers of A. van Leeuwenhoek. — The Royal Academy of Sciences of Amsterdam has decided to issue a critical edition of the collected papers of ANTHONY VAN LEEUWENHOECK.

Anybody who is able to give information about manuscripts of LEEUWENHOECK or about other documents or objects relating to this great naturalist is earnestly requested to communicate with the chief editor, Professor E. C. VAN LEERSUM (Vondelstraat 28, Amsterdam-West, Holland).

Prix Eugenio Rignano.—La Revue « Scientia », dans le but de rendre le plus beau tribut à la mémoire de son ex-Directeur, en donnant une impulsion nouvelle aux études scientifiques et philosophiques auxquelles il a consacré toute sa vie, et en développant en même temps, et de plus en plus, cette union, cette émulation spirituelle entre les savants de tous pays, qui fut sa plus haute aspiration, a institué un *Prix Eugenio Rignano*, de *dix mille Lires italiennes*, à décerner par *Concours international* à l'auteur, jugé le plus digne, d'une étude sur le sujet suivant : *L'évolution de la notion du temps*.

Conditions du Concours :

1. Les aspirants au Prix devront faire connaître leur intention de prendre part au Concours, en envoyant leur ouvrage à la Rédaction de « Scientia » au plus tard le 31 décembre 1932;

2. Les ouvrages doivent être inédits ou publiés après l'année 1930; ils doivent être écrits en une des langues suivantes : italien, français, anglais, allemand, espagnol; les ouvrages non imprimés devront être écrits à la machine;

3. En tout cas, chacun des ouvrages doit être accompagné d'un résumé écrit à la machine, qui n'ait pas plus de 10 pages (4000 mots) et qui puisse être publié comme article dans la Revue.

4. L'examen des ouvrages envoyés sera confié à une Commission nommée par la Direction de la Revue.

Pour tout renseignement, s'adresser à la Rédaction de « Scientia », 12, Via A. De Togni, Milano (116), Italia.

LA DIRECTION DE « SCIENTIA ».

3. Personalia.

Goichi Sawata.—Mr. GOICHI SAWATA, author of a history of Japanese mathematics in Japanese (1928) died on March 12, 1931.

Toyota Takanuki.—The Japanese mathematician TOYOTA TAKANUKI died on April 14, 1931, aged 75. He was deeply versed in the history of Japanese mathematics. He was the late ENDÔ's brother-in-law.

Y. MIKAMI.

Heinrich Wieleitner. — Le grand historien des mathématiques, HEINRICH WIELEITNER, est mort à Munich après de longues souffrances le 27 décembre 1931. La revue *Isis* perd en lui un ami de la première heure et un de ses meilleurs collaborateurs. Elle adresse à la famille du cher disparu ses plus sincères condoléances. Une biographie écrite par le Prof. JULIUS RUSKA paraîtra dans un prochain fascicule.

G. S.

Reviews

A. Kammerer.—*Pétra et la Nabatène*. L'Arabie pétrée et les Arabes du nord dans leurs rapports avec la Syrie et la Palestine jusqu'à l'Islam. 2 vols. vol. 1, texte : 620 p., 7 pl., 4 cartes, 74 fig.; vol. 2, atlas : 152 pl. Paris, Geuthner, 1929 (300 frs.).

Nous avons parlé il y a quelques années de l'*Essai sur l'histoire antique de l'Abyssinie* du même auteur (Paris, 1926; *Isis*, 9, 449-50). Le présent ouvrage est plus important, mais comme il ne ressortit qu'indirectement à notre domaine, nous ne pouvons en parler aussi longuement qu'il le mérite. C'est une admirable synthèse d'histoire antique (et médiévale) et de géographie historique, qui est d'autant plus méritoire qu'elle présentait des difficultés considérables. Il reste d'ailleurs beaucoup de problèmes à résoudre; ainsi l'histoire du proche orient à l'époque hellénistique est d'une complication folle (p. 119). Les ruines de Pétra furent vues pour la première fois depuis le Moyen âge par JOHANN LUDWIG BURCHHARDT en 1812, mais elles ne commencèrent à être étudiées qu'en 1895. Les Nabatéens vinrent probablement dans ce pays du sud-est de l'Arabie; il n'est pas douteux « qu'ils ne fissent partie intégrante du groupe inextricablement apparenté de toutes les races habitant la Syrie méridionale à l'aube de l'histoire » (p. 31). Leur pays, plus ou moins confondu avec Édom, est compris entre Madian au sud et Moab au nord. Les Arabes de l'Hedjaz leur sont probablement apparentés. La civilisation nabatéenne est caractérisée par une langue spéciale du groupe sémitique. L'alphabet est d'origine phénicienne; son étude ainsi que celle d'inscriptions similaires (par ex. celles découvertes au Sinaï par FLINDERS PETRIE en 1906) a ranimé les discussions éternelles sur les commencements de l'écriture (bon résumé, p. 441-9). Il y a un grand nombre d'inscriptions nabatéennes non seulement dans leur pays, mais parfois fort loin, par ex. à Pouzzoles près de Naples. (p. 450-68). Je signale encore un aperçu des cartes anciennes de l'Arabie, et de la Palestine (p. 290-4).

L'influence égyptienne ne se fit jamais sentir en Nabatène que sous la forme ptolémaïque. Une série de rois nabatéens régnèrent de 169 av. J.C. environ à 106 ap. J.C. En cette année, le 22 mars (p. 260) le pays fut

annexé à l'empire romain et englobé dans la Provincia Arabia. Une ère nouvelle, l'ère de Bostra, date de cette année 106. Sous TRAJAN, donc entre 106 et 117, une nouvelle voie romaine relia Bostra à Jaffa via Philadelphie *(Via nova a finibus Syriae usque ad mare Rubrum)*. Les Romains furent obligés de créer une *limes* arabe défendue par un système de 25 à 30 postes dont quelques uns furent de véritables camps retranchés. Cependant la prospérité de Pétra ne dura guère : Bostra dans le Hauran l'avait remplacée comme capitale dès le début de l'occupation romaine ; il est vrai que l'on construisit à Pétra un immense théâtre (milieu du IIe siècle), mais les travaux de construction étaient déjà arrêtés dans cette ville au milieu du IIIe siècle. Qu'arriva-t-il ? Simplement que les routes des caravanes reliant l'Inde à l'Europe se déplacèrent vers le nord : la route passant par la Nabatène, le Sinaï et l'Egypte fut remplacée par une autre passant par Tadmor (Palmyre), Damas, et Beyrout.

« Le commerce de l'Inde semble avoir pris alors la direction du golfe Persique. Sans cesse, à travers les siècles, selon que la sécurité est mieux assurée, soit dans ce dernier golfe, soit dans le *sinus arabicus*, nous assistons à un mouvement de bascule de la route du commerce de l'Inde. Tantôt elle suit l'Euphrate, tantôt elle se dirige par mer vers l'Egypte. Et cette alternance se continuera à travers la période arabe, le moyen âge et la renaissance. Elle se traduit, sous les Ommayades, par la prépotence de Bagdad ; au XVIe siècle, par la compétition des Vénitiens et des Portugais en mer Rouge ; au XVIIIe siècle, par l'importance de l'entrepôt d'Alep. » (p. 309).

Pétra fut de plus en plus délaissée, et devint même un lieu de déportation. FLAVIEN, évêque d'Antioche y fut relégué, et THÉOPHANE nous parle d'un *alchimiste* qui y fut déporté par l'empereur ANASTASE.

Le chapitre XVII (347-67) raconte les vicissitudes de Pétra, alors nommée el-Aswit, à l'époque des Croisades, — mais cela est une autre histoire, fort intéressante d'ailleurs, qui nous mènerait trop loin.

Il y a une bonne description des tombeaux de Pétra et surtout du chef-d'œuvre de cette civilisation hellénistique, le Khaszné (Trésor) que l'on découvre soudainement après avoir été enfermé pendant longtemps dans un défilé profond. Ce temple (d'Isis ?) ou tombeau date probablement de la dynastie des rois de Nabatène (OBODAS III ? 30-9 av. J.C.) ; mais certains savants ne le placent qu'au IIe siècle après J.C., même après la visite de HADRIEN à Pétra en 131 ; d'après eux HADRIEN en aurait ordonné la construction. La date plus ancienne me parait plus plausible.

L'auteur raconte fort agréablement sa propre visite à Pétra et Kérak ; parti de Jérusalem, il se rendit à Amman et traversa la Transjordanie dans toute sa longueur (Pétra est maintenant en Transjordanie mais

tout près de la frontière du Hedjaz). J'ai lu ce récit avec d'autant plus d'intérêt que je compte bien renouveler la randonnée de l'auteur au printemps de 1932.

Cet ouvrage très riche et fort bien indexé est complété par d'excellentes cartes et un admirable album de 152 planches qui forme un véritable corpus iconographique. Étant donné cette richesse le prix est modeste. Nous attendons avec impatience le grand ouvrage du même auteur sur La mer Rouge, l'Abyssinie et l'Arabie depuis l'Antiquité.

G. S.

Léopold de Saussure.—*Les origines de l'astronomie chinoise*. x+594+iv p., 39 fig. Paris, Librairie orientale et américaine, MAISONNEUVE Frères, 1930. (150 fr.)

PAUL PELLIOT has given, in *T'oung Pao*, 24, 296-300, 1926, an essentially complete list of the writings of LÉOPOLD DE SAUSSURE (1866-1925). Of the 41 items of this bibliography, about two thirds are devoted to the calendar, chronology, cosmology, and astronomy of the Chinese; the remaining third consists of papers of a purely historical or philological nature. Although many of L. DE SAUSSURE's papers on Chinese astronomy and related subjects appeared in the *Archives des sciences physiques et naturelles*, the *Journal asiatique*, and the *New China Review*, the most imposing group can be found in *T'oung Pao*, vols. 8 to 23, from 1907 to 1924.

The book under review informs the reader, on the verso of the title-page, that it contains a « reproduction photomécanique posthume d'articles parus dans le *T'oung Pao.* » As a matter of fact, the volume contains the incomplete reprint of *Le texte astronomique du Yao-Tien*, and a slightly mutilated reprint of the unfinished series of articles, *Les origines de l'astronomie chinoise*. One looks in vain for a reprint of *Le cycle de Jupiter* (*T'oung Pao*, 9, 455-75, 1908), a paper not superseded, in 1913 and 1914, by the two instalments of chapter G bearing the same title but treating the subject in quite a different manner. There is no reprint of the short but interesting note, *La Tortue et le Serpent* (*T'oung Pao*, 19, 247-48, 1920). The absence of a reprint of the first part of *La chronologie chinoise et l'avènement des Tcheou* (*T'oung Pao*, 23, 287-346, 1924) is due, we may assume, not to the negligence of the editor but to the historical rather than astronomical nature of the paper; incidentally, the publication, in *T'oung Pao*, of the second part of this paper was promised, in 1926, by P. PELLIOT.

I have mentioned the missing reprints, in order to prepare the reader of this review for a closer scrutiny of the two reprints which bear positive —not negative—marks of the editor's haste. The reprint of the first

paper, *Le texte astronomique du Yao-Tien*, is incomplete. The original paper (*T'oung Pao*, 8, 301-90, 1907) was followed, in the same volume of the periodical, p. 559-61, by an important *Note rectificative et complémentaire* which was overlooked by the editor. The reprint of the series of papers, *Les origines de l'astronomie chinoise*, ends abruptly with section VIII of the first part of *Le zodiaque lunaire* (1922), and the reader is not even told that, according to P. PELLIOT (*T'oung Pao*, 24, 298, 1926), « la suite n'était pas rédigée à la mort de l'auteur. »

The pagination of the book under review is another instance of editorial superficiality. The paper from which the title of the book is derived, *Les origines de l'astronomie chinoise*, consists of the following nine chapters : A. L'origine des sieou; B. Les cinq palais célestes; C. La série quinaire et ses dérivés; D. La série des douze « tche »; E. Le cycle des douze animaux; F. La règle des cho-t'i; G. Le cycle de Jupiter; H. Les anciennes étoiles polaires; I. Le zodiaque lunaire. LÉOPOLD DE SAUSSURE's cross references mention, as a rule, these chapter letters followed by the corresponding page numbers, or the year—not the volume—and the page number. It is obvious that the editor should have added, at the head or at the foot of each page, the year, the chapter capital, and even the volume. The editor or publisher introduced, instead, a continuous pagination; he did not, however, add his new page numbers in brackets : he went to the trouble of deleting the original pagination and of replacing it by his own. On p. 375 he went still farther : footnote 2 was promoted to the rank of footnote 1, both in the text and at the bottom of the page; the eliminated original footnote 1 contained, in addition to a set of cross references, the correction of an error; is it necessary to add that the person responsible for the mutilation of p. 375 of the so-called photomechanical reprint neglected to correct the error on p. 347 mentioned in the suppressed footnote? According to ALDO MIELI (*Archeion*, 13, 109, 1931), the publisher has inserted in his review copy a loose leaf with a concordance table of pagination; the copy which fell into my hands has no such loose leaf. For the benefit of the readers who are in the same predicament, I reproduce below my own table of pagination; the corrections in the 3rd column have to be added to the page numbers given in L. DE SAUSSURE's cross references, in order to obtain the page numbers of the MAISONNEUVE reprint.

Chapter	*T'oung Pao*	Corrrection	MAISONNEUVE
—	VIII, 301-390, 1907	--300	1- 90
A.	X, 121-182, 1909	— 30	91-152
B.	X, 255-305, 1909	—102	153-203
C.	XI, 221-292, 1910	— 16	205-276
D.	XI, 457-488, 1910	—180	277-308

E.	XI, 583-648, 1910	—274	309-374
F.	XII, 347-374, 1911	+ 28	375-402
G.	XIV, 387-426, 1913	+ 16	403-442
G.	XV, 645-696, 1914	—202	443-494
H.	XX, 86-116, 1921	+409	495-525
I.	XXI, 251-318, 1922	+276	527-594

The ten-page « préface » is signed by GABRIEL FERRAND, Ministre Plénipotentiaire. It includes, among other quotations, a reprint (from the *Journal des Savants*, 512-16, 1908; *T'oung Pao*, 9, 708-13, 1908) of a review, by PIERRE PUISEUX, of *Le texte astronomique du Yao-Tien* and of three other early papers by L. DE SAUSSURE on Chinese and primitive astronomy; more up-to-date is G. FERRAND's reprint of an important letter addressed by L. DE SAUSSURE to A. LEGENDRE (*La Nature*, No. 2719, suppl., p. 157, 1926). G. FERRAND failed to mention the date of this letter, July 2, 1925; L. DE SAUSSURE died on July 30. I quote the last sentence of this document which is of interest for the student of the evolution of L. DE SAUSSURE's opinions on the origins of Chinese astronomy : « Cet exposé se résume dans la connexion entre nos recherches, lesquelles aboutissent à la constatation d'une *importation en Chine* des éléments de supériorité de la race aryenne (tacitly changed, by G. FERRAND, to « race blanche »). Mais cette importation, d'après les données du système astronomico-cosmologique, est beaucoup plus ancienne que l'on ne le croit généralement. »

Now that the reader is acquainted with the contributions of the editor or publisher and of the prefacer to the volume under review, let us look at the contributions of L. DE SAUSSURE to the history of Chinese astronomy. *Le texte astronomique du Yao-Tien*, a sort of preliminary chapter to *Les origines de l'astronomie chinoise*, ends with the following sentences characteristic of the author and of his methods :

« Avant de réédifier, objectivement, une théorie de l'ancienne astronomie chinoise, il m'a paru nécessaire de faire, au préalable, table rase de toutes les erreurs accumulées depuis soixante ans dans ce domaine. Il faut donc considérer ce qui précède comme un simple travail préalable de démolition et de déblaiement. Sous ce rapport mes conclusions seront nettes :

« Les ouvrages de CHALMERS, LEGGE, SCHLEGEL, RUSSELL, WHITNEY, SÉDILLOT, KÜHNERT et GINZEL, pour autant qu'ils concernent le texte du *Yao-Tien* et l'origine des *sieou*, doivent être conridérés comme nuls et non avenus. Il n'en reste pas, je pense, pierre sur pierre. Si ces auteurs avaient simplement fait fausse route, cela n'aurait en soi rien d'étonnant ni de blâmable; mais ils ont écarté, avec obstination, les judicieux avis de GAUBIL et de BIOT : *Errare humanum est, diabolicum perseverare.* » (*T'oung Pao*, 8, 388, 1907).

During the two decades which followed this orgy of demolition, L. DE SAUSSURE brought together a vast store of valuable material for the rebuilding of the history of Chinese astronomy; this material is deposited—chiefly but not exclusively—in *Les origines de l'astronomie chinoise*. He even began to build with this material, but the need for frequent and extensive alterations soon became obvious. Let us take, for example, the problem of the lunar zodiac, as reflected in the sieou of the Chinese, the nakṣatra of the Hindus, the manāzil of the Arabs.

In the beginning of his studies, L. DE SAUSSURE insisted on the antiquity, and even on the autochthonous origin, of Chinese astronomy; these views can be found in the reprints from *T'oung Pao*, 1907 and 1909. In *Le zodiaque lunaire asiatique* (*Archives d. sc. phys. et nat.*, 1, 105-26, 1919), and in *La symétrie du zodiaque lunaire asiatique* (*Journal asiatique*, 14, 141-48, 1919), he admitted that the nakṣatra represent an earlier form of the original Asiatic system than the sieou of the 24th century. The last paper, *Le zodiaque lunaire*, reprinted in the book under review, remained incomplete, as I have pointed out before; although it appeared in 1922, it does not supersede the two papers of 1919. In the *Origine babylonienne de l'astronomie chinoise* (*Archives d. sc. phys. et nat.*, 5, 5-18, 1923), and in *Le système cosmologique sino-iranien* (*Journal asiatique*, 202, 235-97, 1923), the Babylonian origin of the lunar zodiac is suggested; although the *Origine chinoise de la cosmologie iranienne* (*Journal asiatique*, 20, 302-06, 1922) belongs to a different group of L. DE SAUSSURE's papers, it ought to be quoted here, along with the *Note sur l'origine iranienne des mansions lunaires arabes* (*Journal asiatique*, 207, 166-68, 1925), in order to bring out the complexity of the problem. L. DE SAUSSURE's letter to A. LEGENDRE, quoted above, adds to the confusion.

This sketch of the evolution of L. DE SAUSSURE's views on a subject of major importance for the determination of the origins of Chinese astronomy will serve a twofold purpose.

First, to show that the editor and publisher of the volume under review should not be satisfied with a collection of reprints from *T'oung Pao*: L. DE SAUSSURE's papers which appeared in other periodicals are just as important documents both as material for a history of Chinese astronomy and as records of the evolution of the author's views; I must quote here the synthetic article, *Le système astronomique des Chinois* (*Archives d. sc. phys. et nat.*, 1, 186-216, 561-88, 1919; 2, 214-31, 325-50, 1920), as a more homogeneous presentation of his views than the analytic *T'oung Pao* series, *Les origines de l'astronomie chinoise*.

Second, to show that L. DE SAUSSURE, a pioneer in a new domain of science—comparative cosmology—and the most profound investigator of Chinese astronomy, had the courage of applying his unusual critical

ability both to the work of his predecessors and to the results of his own research. Errare humanum est, diabolicum perseverare.

Harvard Library 189 ALEXANDER POGO.
Cambridge, Mass.

Miguel Asín Palacios.—*El Islam cristianizado.* Estudio del « Sufismo » a través de las obras de ABENARABI de Murzia. Dibujos de CARLOS DE MIGUEL. 541 p., 2 pl., maps. Madrid, Editorial Plutarco, 1931. (35 pesetas).

This beautiful and thoughtful volume reached me unfortunately when I was feverishly completing the proofreading and indexing of Vol. 2 of my *Introduction* and looking forward like a galley slave to the days of freedom, so near and yet so distant. Thus I was certainly not in a mood to read it in the most profitable manner. Yet I will try to give a brief account of it, hoping for the reader's and the author's indulgence if it is not as full and as critical as they would expect.

Don MIGUEL's main thesis is that Muslim sufism was consciously or unconsciously an imitation of the kind of mysticism exemplified by Christian monks. Of course Christian monachism was older by many centuries than Islam, and Muslims came into contact with monks in a great many places. Many of them certainly knew of Christian monasticism and mysticism, but how much did they know of it? Is not mysticism as universal as religion itself? Can one not better explain the similarities of Christian mysticism, Muslim taṣawwuf, and of Buddhist tendencies (not to speak of others) by postulating the existence of identical needs in men of many races, needs which cannot be solved in many different ways? And even if literary expressions of such needs are similar in Christian and Muslim writings, does this necessarily prove imitation? Are not love letters somewhat similar all the world over, and would one suggest on that account that lovers have plagiarized one another? Now mystical writings are nothing but love letters of a special kind.

One would expect mystics to come closer to one another as they attached less and less importance to the letter of their dogmas, and became more and more heterodox, or rather, adox, but again this would prove nothing. It is true most mystics have a disconcerting trick of uniting the most obedient outer literalism with the most extravagant inner freedom. The author asks himself the essential question : is there in Islam a kind of spirituality analogous to that which obtains in Christendom? I cannot follow him in the theological argument involved. According to him the Jansenist proposition « *Omnia infidelium sive peccatorum opera sunt peccata* » is heretical; on the contrary « *Facienti quod est in se Deus non*

denegat gratiam ». The old Catholic axiom « *Extra ecclesiam nulla salus* » is thus wrong. A more gracious doctrine was already taught by St. Augustine :

« *Ita salus religionis huius, per quam solam veram salus vera veraciterque promittitur, nulli unquam defuit qui dignus fuit et cui defuit dignus non fuit* ». (1)

It is clear that this book will be of more immediate concern to the historian of religion than to the historian of science yet students of Muslim science will not read it without profit and I warmly advise them to dip into it. The author realizing the impossibility of discussing every aspect of the subject decided very wisely to restrict his task to the elaborate study of a typical case and concentrated his attention on the great Spanish mystic Ibn 'Arabī, in whose familiarity he has already spent a good many years. (2) The book is divided into three parts : I. Ibn 'Arabī's life, whose vicissitudes are very graphically related (p. 31-117); II. His doctrines (p. 121-274); III. Anthology in Spanish translation of relevant texts extracted from his works. (p. 277-518).

The final statement of part II deserves to be quoted because it is a reassertion of the author's main thesis in a new and interesting form. (p. 274).

« Tal es, vislumbrada en la perspectiva de sus últimos influjos, la fecundidad de la espiritualidad cristiana, que investigaciones futuras pondrán tal vez a más plena luz : en el gráfico que haya de esquematizar su vital evolución, dos líneas arrancarán del monacato cristiano oriental : una, bien conocida ya, señala su influjo en la espiritualidad cristiana europea; otra marcará sin titubeos la influencia que también ejerció en la espiritualidad del Islam. Ambas líneas vendrán, por fin, a coincidir (paralelas o quizá tangentes) dentro del solar español, escenario en éste, como en otros aspectos de la cultura medieval, de las más fecundas interferencias. » (3)

(1) Epist. CII as quoted by H. Pinard de la Boullaye, S. J. : L'étude comparée des religions (p. 94, Paris, 1922). I was led to this book by Don Miguel's own (p. 25). A part of Pinard's additional commentary is worth reproducing : « C'est déclarer, non pas que la bonne foi suffit au salut, mais que Dieu ne refuse jamais l'accès à la foi à qui vit selon sa conscience; c'est professer, non pas que toutes les religions sont bonnes, ni seulement qu'il y a du bon dans toutes les religions, mais que, même dans les religions fausses, grâce à des dispositions providentielles dont le détail nous échappe, il peut se trouver des individus qui appartiennent par l'âme à la ' vraie religion ' ».

(2) Ibn 'Arabī was born in Murcia, 1165, and died in Damascus, 1240. See my *Introduction*, vol. 2, 596-98. For Asín's earlier studies on his life and work, four memoirs, Madrid, 1925-28, see *Isis*, vol. 11, 425; 13, 158.

(3) The Spanish mystics referred to are, Ibn 'Arabī on the one hand, and on the other, Santa Teresa (d. 1582) and San Juan de la Cruz (d. 1591). See my *Introduction*, vol. 2, 549.

This stimulating book is completed by good indices and by a valuable glossary of technical terms. The Spanish transliteration of Arabic is so bewildering to foreigners that it would have been perhaps more expedient to classify the terms in the Arabic order (happily every term is given also in Arabic script). However as that glossary covers not more than five pages, the non-Spanish readers might begin by perusing it entirely.

A translation into English is most desirable, and considering the great fondness of Anglo-Saxons for religious discussions it might be very successful.

GEORGE SARTON.

F. S. C. Northrop.—*Science and First Principles.* XIV+299 p. The MacMillan Company, New York, 1931. (Price $ 3.00).

Those who are accustomed to think about the problems of natural philosophy may be put into three classes: (1) those who believe that nature is essentially atomic; (2) those who believe that nature is ultimately continuous; (3) and those who regard either belief as superfluous to scientific thought. The book of Professor NORTHROP is stimulating in the fact that in it he boldly and unequivocally places himself among the atomists and from the maze of modern discoveries selects and emphasizes those things which support this view.

The author has in a number of papers and more fully in the volume under review proposed what he calls the *kinetic atomic* principle of nature. The origin of this principle he finds in Greek philosophy: THALES with his postulate that the real is physical, HERACLITOS with his assumption that the real is motion, and PARMENIDES with his demonstration that the postulates of THALES and HERACLITOS are contradictory unless something else is added to the system. This addition is a *referent* independent of the atomic structure of the physical stuff. Hence the system of Professor NORTHROP consists essentially of three postulates.

The first of these asserts that nature is atomic and the second that nature is kinetic. « How anyone can review this tremendous accumulation of evidence and verified theory dating from the seventh century B. C. to the present moment, » says the author, « and not accept the kinetic atomic theory is difficult to understand. »

The third postulate is more troublesome to state. Some form of absolute referent seems necessary to the author. NEWTON conceived of this referent as an absolute space and an absolute time. MAXWELL, perhaps, regarded it as an absolute ether. EINSTEIN has destroyed the Newtonian conception, removed the necessity from the ether postulate, and has found his referent in an abstract geometry of space-time. With

Professor NORTHROP the referent is to be found in what he calls a *macroscopic atom*. « This theory conceives of nature as made up of a finite number of moving microscopic particles surrounded by one spherical macroscopic atom. The precise nature of the moving microscopic particles can be determined only by intensive analysis in the technical sciences... The macroscopic atom is finite in size and so small, relative to the finite number of moving microscopic atoms, that it congests them sufficiently to produce a general uniform and approximately constant compound structure over macroscopic distances, but not so completely as to prevent the motion of the miscroscopic atoms and the resultant variable relatedness which their inherent motion introduces. »

The specific nature of this macroscopic atom is difficult to grasp. It is brought into the philosophic picture to account for everything that is not essentially microscopic. At one place we read that the macroscopic atom « is the ultimate unifying organizing principle in nature », at another that it « is not merely eternal and, hence, necessary; it is also the form of an individual physical substance, » and again « that there must be consciousness in the macroscopic compound nature » which the author interprets to mean that the macroscopic atoms « possess bare indeterminate experienced quality in addition to their physical and formal properties ». Other convenient functions of this universal principle will be pointed out below.

The entire volume is written to support the thesis that nature is kinetic atomic. It begins with a review of the three Greek aspects of nature, (1) the physical theory, (2) the mathematical theory, (3) the functional theory. The first of these implies that the real is physical, the second that the real is *being* and that it is rational, the third that the real is *becoming*, that is to say it implies a teleological principle and emphasizes the world of sensation. The second chapter summarizes the theory of relativity and the third the development and principles of the quantum theory, wave mechanics, and the related subject of thermodynamics.

The fourth chapter considers the living organism. It is interesting to note that the author has selected as the basis of his discussion the well known treatise of L. J. HENDERSON on « Blood ». In this he employs HENDERSON's nomogram which expresses the functional relationship which exists between the concentrations of six fundamental constituents of the blood. The author postulates that one can extrapolate to the general nature of a living organism from a single specific but characteristic physiological process. His conclusion after presenting the necessary technical details of HENDERSON's study is that a living organism is essentially atomic and kinetic and also gives evidence in

support of the macrocosmic atom. It is interesting to see how this last conclusion is reached. Because nature has set up in the blood a mechanism for keeping the various concentrations balanced under the varying conditions of life, Professor NORTHROP argues that there must exist a « principle of the conservation of average density ». In order that this average density may be maintained he argues for the existence of « a huge hollow spherical atom surrounding all the traditional moving microscopic atoms of the whole of nature ».

The fifth chapter considers man. In the postulate of the genes the author finds support for his micro-atomic theory; in the rational nature of man he finds evidence of the changeless spherical form of the macro-atom. The concluding chapter entitled : « The Foundations of Experience », introduces a teleological attribute for the micro-atom and the author finds a kind of intelligence in its fundamental nature which he uses to explain aesthetic and religious experience.

It is to be regretted that Professor NORTHROP gives perhaps insufficient attention to the arguments of those who advocate a continuous framework to nature. In the first chapter he summarizes the growth of the wave theory of light and its fruition in the electro-magnetic theory of MAXWELL. The heart of present perplexities is still to be found in the great successes of these theories. Diffraction continues to find its explanation in terms of waves although it is no longer quite proper to ask : « Waves in what ? » DIRAC, SCHRÖDINGER, and the other proponents of the wave mechanics find it sufficient to throw perplexities back upon the activities of a kind of probability function, continuous as far as its mathematical definition goes, which is associated with each light quantum. The atomic nature of light does not dominate modern concepts any more than does the continuous behaviour exhibited in diffraction. It is interesting to see how Professor NORTHROP can retain a complete atomistic faith in view of these difficulties. The convenient macroscopic principle is invoked and we read : « The wave or macroscopic aspect and the corpuscular or microscopic aspect are both parts of a single system, and neither can be defined in terms of the other ». It is here that we encounter difficulty. Nowhere is the exact nature of the macroscopic atom specified. It appears nebulous. Its nature is not exhibited by means of equations nor its attributes formulated in a consistent set of definitive postulates. This is, of course, the main objection advanced against the older ether. It only positive attribute was its property of undulation and this has seemed insufficient. No consistent set of postulates was ever set down which made the ether isomorphic with an actual physical content. The macroscopic atom or principle appears to be an even more elusive entity.

The author, it is to be regretted, has attributed decadence during the Middle Ages to the dominance of the mathematical philosophy of the Greeks. It should be explained that the words « mathematical philosophy » connote with Professor NORTHROP an idealism which attains fixed and immutable forms. Nature is essentially rational and hence its activities may be embodied in a fixed logical framework. Hence mathematical philosophy is not willing to admit the necessity of observation since it assumes its own inner perfection. As a matter of fact nothing is further from the spirit of mathematical philosophy and the modern scrutiny of first principles in logic shows that mathematicians are fully aware of the inherent weakness of their foundation. On an early page the author mentions the work of BROUWER and HILBERT in the foundations of mathematics and one can but regret that he did not show how his own philosophy would regard the statement of the former that mathematical intuition is « fusion of continuous and discrete, a possibility of conceiving simultaneously several units, connected by a ' between ' that cannot be exhausted by the interpolation of new units ». In this problem of the continuum, as yet unsolved, one finds the fullest probing of the dispute between the atomic and the continuous.

Professor NORTHROP, after emphasizing the dominating importance of the world of sensation and experiment, gives the following warning : « Certain contemporary scientists, who are trying so valiently to convince themselves that their present collection of mathematical formulae, which possess no physical meaning, constitutes an ideal state of affairs, will do well to reflect upon this bit of history ». This seems a partial misunderstanding of the rôle which mathematical physics has played in man's progress toward knowledge. FARADAY's discoveries led to the statement of numerous mathematical theories which were later proved to be in contradiction with other experiments. Mathematical physicists continually modified their original equations, however, to meet the new facts; certain constants of elasticity, torsion, density, etc. disappeared and finally the beautiful equations of MAXWELL were evolved. This mathematical ideal, being in agreement with facts, was pushed to its logical consequences and Hertzian waves were discovered. This success of MAXWELL also led to a postulated ether which MICHELSON and MORLEY failed to discover. But this misfortune later evolved the theory of relativity and when this was understood it was then discovered that the question of the invariance of the MAXWELL equations would have led to the relativity postulate many years before. This shows that the mathematical ideal, when it is once well put, is not sterile. Other instances of this process are cited by R. D. CARMICHAEL in his recent volume on « The Logic of Discovery ».

The book of Professor NORTHROP is stimulating throughout and is written with a pleasing style. The resumé of current theories and discoveries has been carefully done. The six chapters are the essentials of six lectures which were delivered on the Deems Foundation at New York University in May 1929.

(Indiana University.) H. T. DAVIS.

Joseph Needham.—*The Sceptical Biologist.* W. W. NORTON & Co., New York, 270 p., 1930.

TITCHENEV hat die Biologie einmal die charakteristisch moderne Wissenschaft genannt. Das war, als es geschrieben wurde, fast noch ein prophetisches Wort, heute aber beginnt es, sich in ungeahnter Weise zu verwirklichen. Überall sind bedeutende Kräfte am Werk, das in der gesamten Naturwissenschaft bisher dominierende physikalische Weltbild, die sogenannte mechanistische Naturwissenschaft zu entthronen. Wohl die stärkste Unterstützung hat ihr vitalistischer Gegner neuerdings im Lager der modernsten theoretischen Physik selbst gefunden. Wenn diese sich von ihren Kausalitätsschmerzen nicht anders zu befreien vermag als dadurch, dass sie mit HEISENBERG (Die physikalischen Prinzipien der Quantentheorie, Leipzig 1930, S. 2) verlangt: « Insbesondere muss bei der Diskussion irgendwelcher Experimente die Wechselwirkung zwischen Objekt und Beobachter berücksichtigt werden, die mit jeder Beobachtung zwangsläufig verbunden ist », dann wird dadurch in die physikalische Theorienbildung selbst ganz zweifellos ein vitales, um nicht zu sagen vitalistisches Motiv hereingetragen, das unvoreingenommen denkenden Biologen und Physiologen (J. S. HALDANE und J. v. UEXKÜLL z. B.) seit langem vertraut ist.

Doch würde nichts verkehrter sein, als nun ebenso einseitig dem Vitalismus sich zu verschreiben. Beide Grundrichtungen biologischen Denkens, der Mechanismus und der Vitalismus, sind nicht nur deshalb aufeinander angewiesen, weil jeder theoretisch von der Existenz des anderen lebt, sie haben sich auch in der ganzen bisherigen Ideengeschichte der Biologie in regelmässigem Wechsel abgelöst und der biologischen Theorienbildung damit jene Weite und Spannung verliehen, die zu immer neuen grossen Synthesen anregt und die biologische Forschung so vor scholastischer Erstarrung bewahrt. Dieselbe wertvolle geistesgeschichtliche Funktion werden sie auch künftig noch auf lange hinaus zu erfüllen haben, auch wenn sich, wie es scheint, beide neuerdings zu einer höheren Synthese wirklich zusammenzufinden scheinen. So wenigstens verstehe ich die nach DRIESCH vor allem von J. S. HALDANE und J. v. UEXKÜLL inaugurietren Versuche, jenseits vom eigentlichen Mechanismus und Vitalismus eine rein *organizistische* autonom-biolo-

gische Weltauffassung zu begründen, in deren Rahmen das bisherige physikalische Weltbild genau so als vereinfachter Grenzfall seinen Platz finden wird, wie die Newtonsche Gravitationstheorie restlos in die Einsteinsche aufgegangen ist.

Bei dieser gegenwärtigen Lage ist nichts wichtiger und wesentlicher, als dass die genannten drei Hauptrichtungen des biologischen Denkens — Mechanismus, Vitalismus und Organizismus — jede fur sich ihre letzten Prinzipien und Überzeugungen so sauber und klar wie möglich herauszuarbeiten versuchen. Soweit der « Neomechanismus » in Frage kommt, haben neben JULIUS SCHULZ (Die Maschinentheorie des Lebens) J. NEEDHAM und ich (Logik der Morphologie, 1926) diese Aufgabe in Angriff zu nehmen versucht. Es ist daher ganz ausserordentlich zu begrüssen, wenn NEEDHAM seine an zahlreichen Stellen verstreuten einschlägigen Arbeiten nunmehr gesammelt unter dem für seine ausserordentlich geistreiche und geradezu amüsante Art, diese Dinge zu behandeln, bezeichnenden Titel : « The Sceptical Biologist » (New York, NORTON, 1930) vorlegt. Man findet in diesem sehr geschmackvoll gedruckten und ausgestatteten Buche folgende Abhandlungen und Essays NEEDHAMS : Prefatory Dialogue; The Sceptical Biologists; The Limitations of Optick Glasses : or, Some Observations on Science and on Religion; Organicism in Biology; The Hunting of the Phoenix; LUCRETIUS Redivivus : or, The Hope of a Chemical Psychology; S. T. COLERIDGE as a Philosophic Biologist; JULIEN DE LA METTRIE; WILLIAM HARVEY and the Witches; ANAXAGORAS : or, Science and Purpose; Materialism and Religion; Bibliography etc.

Ich möchte mich von diesem feinsinnigen Buche nicht verabschieden, ohne noch auf die höchst bemerkenswerten Parallelen hinzuweisen, die gegenwärtig auf dem Felde der theoretischen Biologie in der deutschen und der angelsächsischen Welt bestehen. Alle drei Richtungen — Mechanismus, Vitalismus und Organizismus — haben hier ihre von einander unabhängigen charakteristischen Vertreter gefunden, die nahezu zu den gleichen Schlussfolgerungen gelangt sind. Das ist sicherlich ein ansprechender Hinweis auf den relativen Wahrheitsgehalt jeder der genannten Doktrinen.

(Hamburgische Universität; z. Z. Universidad de Chile in Santiago de Chile.) ADOLF MEYER.

Henry George Farmer.—*The organ of the ancients.* From eastern sources (Hebrew, Syriac, and Arabic). With a foreword by F. W. GALPIN. XXIII+185 p., 16 fig. London, REEVES, 1931 (15 s. 6 d.).

Dr. FARMER's studies on Eastern, and especially on Arabic, music are so abundant, that we have occasions of discussing them in almost every volume of *Isis*. See, e.g., Dr. MACDONALD's review of FARMER's *Historical facts for the Arabian musical influence* (London, 1930; *Isis*, 15, 370-72). A great part of the present volume was already in the press in 1926 but was delayed for various reasons explained in the preface. Many of the facts and documents quoted by the author have been published before. For example, what concerns Jewish music was discussed as early as the middle of the eighteenth century by BIAGIO UGOLINI (1702-75) in his *Thesaurus antiquitatum sacrarum* (Venice, 1744-69), and the BANŪ MŪSĀ and MŪRISṬUS treatises were partly published in German by the late EILHARD WIEDEMANN and FRIEDRICH HAUSER. However FARMER is the first to publish all of these documents in English, and what is more important, to publish them completely and together. The problems involved are very complicated and though the author seems to have done everything which could be done, it cannot be said that they are entirely solved.

The early history of the organ is made specially difficult because of the promiscuous use of the word organum and its equivalents, e.g., 'ugab in Hebrew (Genesis, 4, 21, etc.). When such words occur without technical definition or illustration, it is impossible to deduce anything from their use.

I shall not attempt to follow the author in all the intricacies of his argument, but shall simply try to give the reader an idea of the problems to be solved. Let us begin by defining our terms. What do we mean by « organ »? According to Miss KATHLEEN SCHLESINGER the essential characteristics of such an instrument are « (1) a set of reeds or pipes of various lengths; (2) a contrivance for supplying the pipes with wind and thus enabling them to speak; (3) a system for controlling the distribution of the supply of wind separately to each of the several pipes. » The first and second characteristics were known in very early days, for they were already realized, the first in the reed-pipe and Panpipes, the second, in the bagpipe and bellows. On the contrary the third implied a long evolution in the making of mechanical contrivances. Unfortunately the development of Greek and Hellenistic mechanics is full of obscurity. ARCHIMEDES' contributions are undetermined, the dates of the three other protagonists, CTESIBIOS OF ALEXANDRIA, PHILON OF BYZANTIUM and HERON OF ALEXANDRIA are very uncertain. (1)

The problem is complicated by the fact that three essentially different types of organ were used in ancient times. (1) The *purely pneumatic*

(1) SARTON, *Introduction* (vol. 1, 184, 195, 208-209).

organ wherein the wind supply and the wind pressure stabilization are furnished by bellows or pistons; (2) the *hydraulic organ* wherein the wind supply comes from a hydraulic air compressor; (3) the *hydraulis* wherein the wind supply comes from bellows or pistons, whilst the water only plays the part of stabilizing the pressure. Now the 'ugab of the Old Testament, invented by JUBAL or DAVID, may have been an instrument of the first type; this cannot be proved, but that type was certainly the oldest and in all probability very old, long anterior to the fourth century B.C.; it may have been created in Greece, or in Judaea, or in Mesopotamia (yet there is no Assyrian name for it). The invention of the second type is ascribed to PLATO, ARISTOTLE, ARCHIMEDES, APOLLONIOS OF PERGA, but none of these ascriptions is convincing. The third type, the hydraulis, was probably invented by CTESIBIOS, who flourished under PTOLEMY EUERGETES. Was this EUERGETES I (247-221) or EUERGETES II (146-116)? Following TANNERY, FARMER assumes that it was EUERGETES I, and if that assumption is correct the hydraulis may have been invented before the end of the third century B.C., and the hydraulic organ sometime earlier. Readers should be warned that the author has the unfortunate habit of stating his conclusions far more dogmatically than is justified. For example, he declares (on p. 21): « The pneumatic organ belongs to a period long anterior to the fourth century B.C., and hydraulic organs can definitely be traced to this date, whilst the hydraulis was certainly known in the third century B.C. » The words « definitely » and « certainly » are unwarranted.

The best descriptions of ancient organs are found in Arabic documents. Muslims showed early a deep interest in Hellenistic mechanics, and the relevant treatises of ARCHIMEDES, APOLLONIOS, PHILON and HERON were translated into Arabic before the tenth century. They themselves constructed various kinds of instruments, including the three types of organs mentioned above. The main references to organs in Arabic literature have been carefully collected by the author and are offered by him in English translation. The best description of a purely pneumatic organ was given by one MŪRṬUS or MŪRISṬUS, who cannot be identified. This mysterious author was first quoted by AL-JĀḤIZ (d. 868) and his writings on the organ (urghanun al-būqī, urghanun al-zamrī) were mentioned in the *Fihrist al-'ulūm* (988). From the Arabic point of view his name is singular; it is almost certainly the corruption of a foreign name. Perhaps CTESIBIOS? This hypothesis, first made by Baron CARRA DE VAUX, is somewhat justified by the strange fact that while the Muslims were so eager to translate the Greek treatises on this subject, CTESIBIOS remained unknown to them. Considering the particularities of Arabic script and transliteration, the name CTESIBIOS might have

been gradually changed into MŪRISṬUS (see p. 19). The best description of the hydraulic organ is contained in a treatise entitled « The instrument which plays by itself » (Al-ālat illatī tuzammir binafsihā), which has come down to us under the name of the BANŪ MŪSĀ (see my *Introduction*, vol. 1, 560). Finally the best account of the hydraulis was given in a treatise entitled « Construction of the instrument which MŪRISṬUS invented the sound of which travels sixty miles ('Amal al-ālat illatī ittakhadhahā MŪRISṬUS yadhabu ṣautuhā sittīn mīlān), and a very curious reference to its use in wartime is found in the *Sirr al-asrār*. I cannot resist the temptation of quoting this passage as translated by ROGER BACON (p. 123). The *Sirr al-asrār* was supposed to have been written by ARISTOTLE for ALEXANDER THE GREAT (see *Introduction*, vol. 1, 556).

« Et oportet te tecum habere illud instrumentum quod fecit Temistius ad opus excercitus ad nocendum, et est instrumentum terribile quod dividitur multis modis, quia forte oportebit te vocare totam provinciam tuam et regnum tuum, et congregare subito proceres tuos bellatores tuos in eadem die vel cicius, vel aliquo modo prout indiget excercitus magnus et numerosus, nam hujus instrumenti sonus auditur per miliaria sexaginta. Hoc est cornu eneum artificio mirabili fabricatum, quo ex. LX. miliariis tempore belli suum excercitum convocabat, et regebatur cornu sexaginta hominibus propter sui magnitudinem et inestimabilem artificium, et verisimile est quod multa metallorum resonancium genera in ejus composicione concurrebant, et hec est forma cornu. »

These Arabic treatises of the BANŪ MŪSĀ and MŪRISṬUS are Englished in extenso and illustrated with very curious drawings and diagrams taken from the MSS. A final chapter deals with the introduction of the Arabian organ in Europe : this involves a new series of moot questions which I must forbear discussing. Let me quote a single remark (p. 157) :

« What ultimately contributed to the disuse of the *hydraulis* in Western Europe was precisely the same factor that had led to its supersession after the fifth century, that is to say, the improvement in the weighted blast-bag of the *pneumatic organ*, which provided a more convenient force of stabilisation of the wind pressure, than that involved in the *hydraulis*. »

Appendices contain translations of HERON's description of a hydraulis (in the Pneumatics, extant in Greek), and of ATHANASIUS KIRCHER's description of an automatic hydraulic organ (*Musurgia universalis*, Rome, 1650), an instrument similar to the one constructed by the BANŪ MŪSĀ. An elaborate bibliography and good indices complete this excellent work.

The Rev. Canon FRANCIS W. GALPIN, who contributed an instructive and stimulating preface, distinguished himself some twenty-five years ago by constructing with the help of the late C. F. ABDY WILLIAMS

a working model of a hydraulis. That hydraulis, provided with a manual of nineteen keys and three stops, was actually used by him at a lecture in the Fishmongers' Hall, London, c. 1904, and « came through the ordeal triumphantly even in the solo work ».

Ghubār al-'amal khairum min za'farān al-'uṭlat (The dust of labor is better than the saffron of idleness). This delightful Arabic proverb was chosen by the author as an epigraph presumably to cheer himself up and to serve as a viaticum to the readers. Indeed this is no book for idle men; it will tax the reader's attention to the utmost but not in vain.

GEORGE SARTON.

Alfred Scott Warthin.—*The Physician of the Dance of Death.* A historical study of the evolution of the Dance of Death Mythus in art. XVI+142 p. Frontispiece and 92 figs. in text. PAUL B. HOEBER, Inc., New York, 1931. ((Price $ 7.50).

The series of articles in the *Annals of Medical History* (n.s. vol. 2, nos. 4, 5, 6, 1930, and vol. 3, nos. 1, 2, 1931) are reprinted here in quarto form with additions and corrections. It is a chronological treatment of the Totentanz or Danse Macabre. The *carpe diem*, the philosophy of the ancients, was replaced by the *memento mori* of the Christian faith. Primitive religious drama, religious spectacles, the « Vision Fulbert's, » and « Vado Mori » poems of the 11th and 12th centuries prepared the way for the treatment of death in the graphic arts. The first dated one is that of 1424 in the wall painting in the Cimetière aux Innocents in Paris. During the 15th century this motive of the Dance of Death found expression widely throughout Christian Europe in frescoes in charnel houses, cloisters, and churchyards, in paintings and tapestries, on clocks, drinking cups, and borders of prayer books, and in sculptures. With the coming of printing an immense development followed in wood-cut and copper-plate, culminating in 1530 in HOLBEIN'S masterpiece « Imagines Mortis. »

The author traces and interprets the representations of the physician in successive artists' portrayal of him in this remarkable series of cultural sequences from the mediaeval origins of the Dance of Death down to its recrudescence during the World War and into the after-the-war period of revulsion. The book is an annotated catalogue of the nearly one hundred illustrations with descriptive notes on others not reproduced. In the mediaeval period the didactic religious motive of the theme made the physician in the picture the butt of satire. With the Renaissance and HOLBEIN'S secularization of the subject the satire and caricature are shifted to quackery. The tendency in later times was to replace

the physician by the apothecary and thus to shift the responsibility for failure to conquer death from the physician to the inefficiency of the drugs which he prescribed. The World War poured out a flood of new portrayals of death in which physician and apothecary alike disappear from the picture with the emergence of the motive of displaying the horrors of war. The staying hand of the physician of the Scientific Age on the Dance of Death still awaits its HOLBEIN.

CHARLES A. KOFOID.

Janus. Zeitschrift für Geschichte und Literatur der Medizin im Verein mit L. CHOULANT, H. HAESER, J. F. C. HECKER, C. F. HEUSINGER, F. JAHN, J. C. MARX, J. ROSENBAUM u. A. hrg. von A. W. E. TH. HENSCHEL. 3 vols. Breslau, 1846-48.

Janus. Central-Magazin für Geschichte und Literärgeschichte der Medizin, ärztliche Biographik, Epidemiographik, medicinische Geographie und Statistik. In Verbindung mit mehreren Gelehrten des In- und Auslandes hrg. von H. BRETSCHNEIDER, A. W. E. TH. HENSCHEL, C. FR. HEUSINGER, und J. G. THIERFELDER. 2 vols. Gotha, 1851-52.

Neue Ausgabe mit einer Einführung von Professor KARL SUDHOFF. (5 vols. bound in 4). Leipzig, ALFRED LORENTZ, 1931 (Rm. 185).

The first and second Janus (described in *Isis*, vol. 2, 143) are a real gold mine of medico-historical articles. Though some of them are necessarily a little out of date, the majority are as valuable to-day, and as indispensable to the historian of medicine, as when they were first published some eighty eyars ago. Hence this facsimile reprint of those rare volumes is most welcome.

I welcome it because it is much better to have these precious volumes in any form, than not to have them at all, but this does not mean that these reprints are irreproachable. Far from it! I have often said that inasmuch as photostatic reprints are so easy, no scholar should attach his name to one of them without adding something to the original edition. This new edition shows no trace of editorial care of any kind. For example, the original volumes contain lists of corrigenda (vol. 1, 877, vol. 2, 624, etc.). These corrigenda should have been corrected; it could have been done easily and that was the very least that the editor should have done, but apparently that was already too much for him. The dates of the original parts have been obliterated and thus it is impossible to know when each article actually appeared. It would have been gracious to append a biography and bibliography of HENSCHEL. We are given nothing of the kind. SUDHOFF's preface, which is paraded on the title page as an additional bait, is very short (less than 3 p.) and

as meager and colorless as possible. Almost nothing is said of HENSCHEL himself. (1)

There is a portrait used as frontispiece, and the reader will naturally think: HENSCHEL's portrait! Not a bit. This monument bears no portrait of the great man to whom we owe its existence, but of two other men—SUDHOFF and WELCH. (2) *Sic vos non vobis*... This is as ungracious as possible, almost indecent, and truly ridiculous. I hasten to add that Dr. WELCH had no responsibility in the matter, for these volumes were offered to him as a surprise on the occasion of the inauguration of the WILLIAM H. WELCH Medical Library in Baltimore.

Let us forget as quickly as possible the callousness and the ingratitude which are thus unconsciously displayed in those volumes, and let us think only of their rich contents. To analyze them would extend my review considerably and would only serve to bewilder the reader, instead of which it will be better to publish that inventory in our next critical bibliography. That bibliography will contain notes relative to each article of these five volumes and will thus constitute a systematic table of contents of these volumes. This is our humble homage to the memory of one of the pioneers of the history of medicine, HENSCHEL of Breslau.

G. S.

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(1) A short biography of A. W. E. TH. HENSCHEL (1790-1856) by PAGEL will be found in A. HIRSCH, *Biographisches Lexikon der Aerzte* (vol. 3, 160-161, 1886), but it is unsatisfactory even if one takes its size into account. I would be glad to publish in *Isis* a good biography with bibliography and portrait of that great pioneer and thus help to repair as far as possible the injustice done to him in these volumes.

(2) Needless to say there would be no objection to the presence of these two portraits, on the contrary,—if HENSCHEL's portrait had also been published.

Avis

Étant donné que les prix des livres dans les divers pays d'Europe tendent à s'équilibrer, à partir du 1er janvier 1933 tous les membres de la Société internationale d'Histoire de la Science (History of Science Society) payeront la même cotisation, soit cinq dollars par an.

Toutefois à titre de compromis et pour faciliter la transition, les membres qui n'ont payé jusqu'ici que trois dollars, pourront racheter les cotisations des années 1933, 1934 et 1935 par un paiement de neuf dollars, à condition que ce paiement soit fait *en une seule fois* à M. Guinet (11 Place général Meiser, Bruxelles, Belgique) avant le 1er janvier 1933.

La Direction d'Isis.
31 janvier 1932.